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THE NATURAL HISTORY OF PURPOSE

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Some of the current discussions of purpose seem to shoot wide of the mark, or else perhaps they are shooting at very different targets. An approach to the subject from the biological side by a biologist may help to clear the air and improve the aim. Does a rat show purpose in running a maze? This may be largely a matter of definition of words. The term is commonly applied to certain human activities where the end toward which the act is directed is consciously recognized. Let us start with these human purposes.

The human type of consciousness (which is the only kind that I know anything about at first hand) exhibits, amongst others, two characteristic features: first, human thinking is symbolic and, second, the generalizations expressed by language and other symbols may be so enlarged as to connote more extensive uniformities of experience (laws of nature) which can be projected into the future, thus permitting prediction of future events, adjustment of present behavior with reference to future contingencies, and the fabrication of conscious purposes.

Hunter[1] has analyzed the mechanism of the symbolic process and his analysis in objective terms is all to the good. To be sure, his neurologic schema is purely hypothetical, but it is as good as any other hypothesis and it is helpful in impressing upon us the very elusive fact that every mental act is the function of some special anatomical configuration of parts.

[1] Hunter, W. S., 'The Symbolic Process,' PSYCHOL. REV., 1924, 31, pp. 478–497.

In the neurologic diagram presented by Hunter, a considerable part of the schema is necessarily hypothetical because we do not know all of the connections of the parts which are activated when we say the word 'box' upon seeing this object. But a similar schema designed to illustrate the connections of parts activated when my biceps muscle contracts reflexly in response to a pin prick would likewise have to be filled in by wide stretches of hypothetical construction. We do not know exactly how a sense organ is excited, how a nerve fiber conducts, how a muscle contracts, how a gland secretes, or how the brain thinks, though we have satisfactory evidence that all of these organs do perform the functions mentioned. No biologist with all of the evidence before him can fail to make this deduction. Why some other people accept the evidence in all of these cases except the last and refuse to do so in that one is hard to understand. Presumably it is due to lack of knowledge of the biological evidence [1] or else to a mind fortified against this evidence by prejudice. Such a prejudice may arise from a habit of mysticism, from some incompatible *a priori* philosophical postulate such as parallelism, or perhaps from a phobia against mysticism so powerful as to shy at a shadow cast by a past mysticism on very real experience.

Whether consciousness is present in any particular neurologic process is a fact that can be determined directly in the experience of the subject and its presence or absence is independent of our knowledge of the working of the mechanism. When it is present it is the part of science as well as of common sense to accept it as given—as a datum of experience—in just the same way that we accept sugar tolerance as dependent upon the normal working of the pancreas, though in neither case have we an adequate understanding of the mechanism actually employed. For this we live in hope.

Symbolism, accordingly, can be treated scientifically either (1) in terms of its mechanism, which in the present state of our

[1] Current psychological literature is crammed with illustrations of this ignorance where confusion has taken the place both of mysticism and of established fact, some of which are commented upon in Warren's 'Neurology: Mystical and Magical,' PSYCHOL. BULL., 1923, 20, pp. 438–443.

knowledge must remain largely theoretic, or (2) in terms of the awareness of the symbolic process, where the evidence is direct experience, though the experiencing process is so unique and so refractory to analysis that we have great difficulty in articulating it with other natural processes.

The second characteristic of human consciousness to which reference was made above, namely, the forecast of future events and shaping of present conduct in reference to future contingencies, uses symbolism as its necessary tool. It comes to fruition as conscious purpose. And here again it is easier and more satisfying (in the present state of our knowledge of the nervous system) to experience purpose than to unfold its bodily mechanism.

Symbolism and purpose as natural functions of the human body doubtless have natural origins. There is no evidence that they have always existed in the forms experienced by us or that they exist at all in the newborn child or the lower mammals. But, of course, they are not made out of nothing.

The origin of symbolism may well be conceived along the lines suggested by Hunter. Or, to express it neurologically, the associational fields of the cerebral cortex seem to provide the anatomical mechanism of the process, a mechanism that more than doubles in mass and in internal complexity as we pass from the highest anthropoids to man. The biological origins of purpose can be sought with fair prospect of success in certain very general features of all animal behavior, and to these we will next direct attention.

It is generally recognized that many animal reactions, such as the typical tropism, reflex, or instinct, have both a backward and a forward reference. The forward reference we express biologically by saying that these activities are in general adaptive, that is, the animal tends to react in each frequently recurring situation in fixed modes that favor the preservation of the individual or the species. This teleological behavior is not directed by any awareness of the ends to be attained, but the organization has been so shaped by its past history (the backward reference) that it can do no other.

This backward reference involves no mystic forces. In

evolutionary history and personal development the structure of the body has been shaped by biological processes (some of which are well known) in adjustment with the world in which it lives. The vital process is at basis just this adjustment, and the structure of the body at any moment has been determined by the history of previous racial and individual adjustments. And this structure, in its turn, determines the action system of the individual at that moment.

The action system of the individual is, accordingly, the dynamic expression of his inner nature, and both of these have been biologically determined. The inner nature (that is, the protoplasmic organization) being what it is, the behavior is largely predetermined by the existing structure and reactions to stimulation tend to follow the established patterns—tropism, reflex, instinct—which in general are good for the organism exhibiting them. It is easy, therefore, to see why these tendencies are the strongest kind of internally determined impulsions. The impulse, not being intelligently directed, persists even when in a particular case it may be harmful, as seen when the moth repeatedly sears its wings in the flame.

These impulsions are coeval with life and they have persisted throughout the entire course of organic evolution. They face backward in that they have been shaped by biological agencies, such as natural selection during the previous evolutionary history of the species, or by habit during the individual life. They face forward in that they are adaptive or directed (in general) toward some useful end, to wit, the welfare of the individual or the species.

Reflex behavior in any particular instance is, accordingly, the result of some adequate stimulus acting upon an internal organization which has been so fabricated during its own past history as to predetermine the nature of the ensuing reaction. The stable pattern of the internal structure at the moment is, therefore, a causative factor in determining the dynamic pattern of the behavior. It is not a mystic 'force.' [1]

This primitive sort of impulse is an expression of a definite

[1] Warren, Howard C., 'The Subconscious,' *Scientia*, 1923, **34**, pp. 91–100.

trend or 'set' of behavior patterns in modes which have been established and stabilized in previous racial and individual experience and which, while in actual operation, may draw upon certain biological reserves (present in every organism) and so exhibit a characteristic physiological pressure or 'drive.'

If we apply the expression 'a push from behind' to these hereditary and habitual impulsions, we use figurative language. Neither heredity nor habit is a force, but these are expressions of fixed structural molds through which certain of the vital energies must come to expression (if at all) and by which the character of this expression is determined.

Josey [1] has, in fact, fallen into a grievous fallacy here. In attacking certain appeals to mystical forces as determiners of human behavior (which still persist in some quarters) he goes too far and in effect denies the practical significance of any innate factors in behavior. These innate factors are not extraneous 'forces' or metaphysical agents; they are stable patterns of protoplasmic activity, and as such, they are true causative factors in behavior.

The driving power of impulse is derived not at all from mythical hereditary 'forces' and in very small measure from the physical energy of the stimulus which sets off the impulsive process. The internal sources of this impulsive energy are of two sorts. First, there are the general bodily reserves of energy in excess of immediate needs normally present in every healthy organism, to which in higher animals there are added elaborate devices for step-up of energy within the central nervous system, as in the 'avalanche conduction' of the cerebellum.[2] These are knit in with the stereotyped action systems and supply the impulsive energy or 'drive' of all reflex and instinctive behavior.

The second kind of apparatus of neural reserves is well developed only in the higher vertebrates and preëminently in the association centers of the human cerebral cortex. It is

[1] Josey, C. C., 'The Social Philosophy of Instinct,' New York, 1922.

[2] Herrick, C. Judson, 'Neurological Foundations of Animal Behavior,' New York, 1924, pp. 263, *ff.*

radically different from the other. The first sorts of reserves support reaction patterns which, in their broad outlines certainly, and apparently often in detail, are laid down within the innate organization and are with difficulty modified (in lower animals) by personal experience. Here the associated reserves in general reinforce, prolong, or otherwise facilitate the reflex and instinctive behavior. The second sorts of reserve mechanisms, on the other hand, are superposed upon the innate apparatus (not knit into it); they mature much later (wholly postnatal in the human infant), and the patterns of their development are largely shaped by individual experience. They are functionally knit in with individually modifiable behavior.

Reflex and instinctive action function largely in terms of racial experience. The association centers, on the other hand, form in the aggregate the great storehouse of personal memories, the residua of individual experience. These centers contain countless nerve cells with short axons (type II neurons) whose connections are such as to permit their activation in relation with each one of numerous different associational patterns, thus making possible a great influx of nervous energy in addition to that of the neurons which form the essential links in the particular associational chain at the moment active.

These cortical reserves are drawn upon, not like those of the lower brain centers to reinforce the administrative machinery of structurally predetermined reactions, but to coöperate in determining on the basis of personal as well as racial experience what act is appropriate to the present situation. This is a distinction of far-reaching importance. It is the presence of the cortical reserves, partly unformed and mobile and partly laid down as definite mnemonic patterns or engrams, that makes constructive thinking possible, a process in which residua of former experience are reassembled in new patterns.

Now, colligated with the amplification of the cortical apparatus for registering and reactivating the mnemonic and other reserves of the human associational fields, we observe the

elaboration of symbolic thinking and the emergence of new capacities to formulate uniformities of experience, to project these uniformities into the future, and so to predict coming events.

This forward projicience of experience is not a new and mystical 'force' added from the outside to our biological impulses to transform them into voluntaristic purposes. On the contrary, since all mental acts are functions of the nervous system, human purpose and choice in view of probable future contingencies are simply functions of a different sort of nervous system from that tiny speck of nervous matter which sets the direction of the insect's reflexes.

The forward reference of reflex and instinctive behavior, its biological adaptiveness, is not in lower animals so far as we know accompanied by any 'mentalistic something' that in our own experience we identify as purpose. If we choose to give the name purpose to any 'toward-whichness' of behavior, as Tolman [1] does, this is a matter of definition of terms which need not here be debated. We agree that this type of animal behavior which persists until some definite result is attained can be described adequately for present scientific uses without any mentalistic complications. But the purpose of which I am speaking here is that sort of human behavior which is consciously directed toward an end that is more or less clearly formulated in mental terms. And I maintain that when this awareness of ends to be sought is present and is a component factor in a causal sequence of action it is not a negligible element in a behavioristic account. And that it is present in some of my own choices, I have direct evidence as a datum of experience.

This power of conscious choice in the light of probable future contingencies, again, has not grown up out of nothing. It is an amplification of the 'forward reference' of those reflex and instinctive reactions already considered. The 'end' which in the reflex is attained blindly is here purposefully striven for. Even in the simplest verbal symbolism the internal apparatus of this forward reference is far more elabo-

[1] Tolman, E. C., 'Behaviorism and Purpose,' *J. Phil.*, 1924, 22, pp. 36–41.

rate (especially in its social implications) than in the most complex instinct, as Dewey [1] has shown. The significance of this in behavior has been overlooked in Hunter's over-simplified schemata.

My judgment of the probable course of future events and my forecast of the probable consequences of my own proposed action are determining factors in shaping a decision or 'making up my mind' to a purposeful choice just as truly as are my hereditary predispositions, my established habits and my temporary physiological condition of hunger, fatigue, depression, or exuberance of vital tone—and they may be by far the most significant factors.

Every cortical process is a natural result of previous bodily activities. It is in turn a cause of subsequent bodily activities. This cortical process may or may not be a conscious act. Its antecedent causes may be previous mental processes or events of simpler physiological sorts; its results may be mental acts, muscular movements and events in the surrounding physical world. If the cortical process is a mental act, this is a fact, a datum of experience, the neglect of which leaves the causal sequence incomplete.

Let us suppose that I am debating whether to spend the ten dollars that I have just received as a birthday present upon a dinner with my wife at a fashionable restaurant or upon a book that we have long wanted to read together. I set over against a gay evening amid the bright lights the prospect of many quiet evenings at home with a favorite author and I cannot make the decision until I recall that I am at present convalescent from an illness. The prospect of a gastric upset following injudicious eating decides the matter and I determine to buy the book.

Every step in the process of making this decision and forming this purpose is causally determined and the choice actually adopted is the mechanistic expression of the working of my internal organization at the moment of its exercise. The factors which have operated in the fabrication of this inner nature whose expression is the purpose are various in the

[1] Dewey, J., 'Knowledge and Speech Reactions,' *J. Phil.*, 1922, **19**, pp. 561–570.

extreme—hereditary disposition, habit, countless memories, etc., all facing backward, and in addition to these the rational analysis and evaluation of the probable future consequences of each of the two lines of conduct under consideration. This mental act, whose neurologic mechanism I can picture in only the vaguest outlines, is unquestionably the dominant factor in the causal complex. Why not recognize it in its own right for what it is and put it in its proper place in the causal sequence? And clearly the fact that it is *my* stomach that will ache if I overeat and the emotional pulse that wells up in my consciousness at the anticipation of such a result can be evaluated and treated scientifically as truly—though not as completely—as if I knew all of the neurologic and endocrine activities that condition this emotional pulse.

And this 'vital, personal quality of experience' which Lashley [1] would throw out of science into art or mysticism is a real datum of experience which is not esoteric or mystic unless we choose to make it so. The demand that we evaluate scientifically subjective experience by exactly the same criteria as are adequate in physics and chemistry is a thoroughly unscientific procedure, for the differentia of mind as I experience it is just this 'personal quality' that is (for me) absent in those natural processes that I objectify. Some differentiating features are characteristic of every field of experience that we recognize in our classification of the sciences—biology, astronomy, chemistry, etc. We do not insist that the data experienced in these fields shall be identical, but we do insist that they shall be congruous and that they knit together in a unitary system of nature. And my mind does knit in with the rest of my living in just this way.

The various departments of experience (the 'sciences') are differentiated partly in terms of the kinds of things that evoke the experience, partly in terms of the technique employed in enlarging this experience, and partly in terms of the interest or attitude of the experiencing subject. To conform to our present canons of scientific validity, all of these experiences must

[1] Lashley, K. S., 'The Behavioristic Interpretation of Consciousness.' PSYCHOL. REV., 1923, **30**, pp. 237–272, 329–353.

cohere in a unitary system of cause-and-effect sequences. This way of looking at the cosmos has grown up within scientific experience. It is a scientific, not a philosophic dogma. In fact, in many circles it is in disrepute, on philosophic, religious, or sentimental grounds, not on scientific grounds.

There are three fields in which this scientific dogma presents especial difficulties—life and death, mind and matter, and the current doctrines of relativity—and mysticism has been invoked in each of these fields without advancing our understanding of them in the slightest degree. The whole trend of science is in the direction of integration of experience and in the light of history there is nothing to be gained at present by throwing overboard the scientific method. This method demands that we endeavor to fill existing gaps in experience rather than adopt easier short cuts to a solution through pluralism, a solution which in the end resolves itself into a name for our ignorance.

That the experience which I do not objectify is causally knit in with that experience which I do objectify and call natural phenomena I regard as scientifically established by the same kind of evidence as is ordinarily applied to other bodily functions. That this subjective experience, which I call thinking and feeling, is congruous with the rest of nature is not so evident, and this indeed has been the great stumbling block in the entire history of the body-mind problem. But there are numberless other instances in natural science where the lack of apparent congruity of phenomena is equally patent, though not so disturbing. Indeed we have only vague hints of the underlying reasons why the chemical elements exhibit their respective peculiar properties of color, chemical affinity, melting point, etc.; yet we do not doubt that they form a natural system. The fact that this field is being rapidly clarified heartens us, though it is a far cry to a successful outcome when we essay to knit either life or mind into this same unitary system. Vitalism and spiritism do not offer the most promising approach to the problem. In this we agree with the most thoroughgoing behaviorists. Of course, this way of looking at life does not solve out of hand all of the

philosophical problems suggested by living. But fortunately we do not have to await these solutions in order to live happily and well, nor indeed in order to develop a very serviceable science of the vital processes.

Wheeler [1] has endeavored to show that objective behavior and introspective experience are not as disparate as is commonly assumed. "Introspection does not differ in kind from other methods of scientific observation. . . . Introspection is as behavioristic a procedure as is watching a rat in a maze." On the other hand, Lashley has reviewed critically the literature of introspection and, out of the mouths of the introspectionists themselves, he has assembled an astonishing array of evidence of the futility of these historic attempts to analyze and explain conscious experience. We admit that the phenomena of gravitation are natural processes, though we have not yet succeeded in analyzing them further and the explanations which have been offered are as fantastic as are the explanations of mind.

McDougall [2] says that it is roughly true that all the psychologists who continue to make use of introspection while accepting the mechanistic psychology hold to the belief that our thinking or consciousness is the epiphenomenon of the mechanical processes of the brain. Possibly a statistical survey would not bear this out; but at any rate the scientific status of the group of psychologists to whom he refers is not unlike that of some hypothetical cult of physicists who accept all of the properties of matter except gravitation but hold that this is an epiphenomenon of no causal significance and hence negligible in physical science though it may be treated in a separate gravitational science in a non-physical realm.

No abyss of ignorance of what consciousness really is, no futilities of introspective analysis, no dialectic, destroy the simple datum that I have conscious experience and that this experience is a controlling factor in my behavior.

Lashley in the work cited (p. 272) has endeavored to show

[1] Wheeler, R. H., 'Introspection and Behavior,' PSYCHOL. REV., 1923, 30, pp. 103–115.

[2] McDougall, Wm., 'Purposive or Mechanical Psychology?' PSYCHOL. REV., 1923, 30, pp. 273–288.

"that as complete an account of the attributes of consciousness can be given in behavioristic terms as can be given in subjective terms as a result of introspective study. . . . The statement, 'I am conscious' does not mean anything more than the statement that 'such and such physiological processes are going on within me.'" He here attempts an impossible enterprise. Even if our account of the physiological processes were perfect and complete objectively, the subjective experience remains a real fact of natural history, not an epiphenomenon or a by-product. There are no by-products in nature. Whether a given product is of value in a particular context is another question. And there are many contexts where consciousness does matter.

When Lashley says in a later passage (p. 336), 'The reactions are awareness,' he appears to be stating exactly my own position and that of Professor Warren and other advocates of functional and double-aspect theories. But the appearance is quickly destroyed when he adds, "The complexes of reaction meet the subjective description of the organization of consciousness, and leave over no undescribed psychic elements." If any possible account of objective behavior does 'meet the subjective description' of consciousness, then the description of that consciousness is surely faulty. And by 'reactions' he perhaps does not mean exactly what either Professor Warren or I would mean by that word in this context.

If our knowledge of physiology were more complete, doubtless we could write energy equations for this entire sequence of events. But energy equations do not tell all that we want to know about any series of natural processes. There are time factors and other matters of pattern of performance that may be very significant.

We know very little actually about the neurologic aspects of symbolic thinking and choice, but we do know that we think and choose and that these acts are causative agents in determining conduct. With us a course of action may be determined impulsively and unwittingly (as in insects), or it may be determined by the same factors which were operative in the

insects plus intelligent analysis of the present situation and of the possible future consequences of each of several different reactions to the situation. The prevision of possible future consequences of action is a real causative factor in determining which course of action will actually be chosen. There is no uncaused action in either case, no mystic 'non-physical forces.' And we really are in a position to know more about the causative factors in the second case than in the first. Our present neurologic knowledge is inadequate to present a complete objective statement of the causative factors in both reflex and deliberative purpose, but in the second case our direct introspective experience can fill some of the gaps. The ideas which have influenced the purpose can be called back and reevaluated, if we so desire.

It is a travesty of scientific method to leave out of consideration in a total view of human behavior just those characteristics which differentiate man from brutes and upon which the further progress of civilization must depend because we do not like to use the only satisfactory data now available for the study of these characteristics.

This argument is shot through with theoretic interpretations of scraps of factual data, some objective, some subjective. I have not hesitated to pass freely from one to the other of these fields in my argument because I am actually doing so in the course of the routine of my daily living. Doubtless I shall be accused of leading a double life. The charge is true. The question is, Does this duplicity rest on a natural or a mythological basis?

Dewey refers to the impossible attempt to live in two unrelated worlds at once, and if my physiologic life and my psychic life are really as unrelated as many people seem to think, then my case is sad indeed, far more so than that of the psychologists whom Lashley chides for 'still precariously bestriding both steeds.' Riding two horses at once is a practicable enterprise, though requiring great skill on the part of both rider and steeds. But as for living in two unrelated worlds, even a very slight disturbance of the normal balance between my simpler physiologic functions and those which are

conscious may give rise to insane delusions and the disruption of the personality. Sanity of both body and mind is this balance.

Lashley's figure of the circus rider is bad. I am not driving the double team. I am the team. Indeed, you may say, if you like, that I am a whole drove of horses (or functions)—various physiologic processes, simple unanalyzed awarenesses of various sorts, awareness of my body as acting, and awareness of myself as experiencing. The numerous attempts to define these processes more precisely have not, so far, been very successful. But the statement of their reality is not mysticism. It is fact. I am a protoplasmic organization observable by others as well as by myself objectively and I also am an awareness which is strictly personal and cannot be shared with another except by an indirection, and that very inadequately.

SOME NONSENSE[1] ABOUT THE 'COMMON PATH'

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In the literature of psychology, in text-books, and in briefer discussions of the latter years, three misleading terms have come to foist themselves, the 'random' responses of the untrained, the 'conditioning' of reflexes, and the 'common path' of the system of conductors otherwise called nervous tissue. They are in ninety-nine cases out of a hundred used where they mislead the reader, and used thus because the author has not made clear to himself the proper relation among the three. That only one of them appears in the title of this article is due chiefly to the fact that long titles are inconvenient.

The oldest of these terms is the 'randomness' of the actions of the untrained, of the young. It probably reached its height of popularity thirty years ago. But it is still popular in the year 1925 in the text-books from which college students of psychology are supposed to get their training in clear and scientific thinking. (It is no secret, of course, that what they chiefly get is training in the *literary use* of psychological terms. Ever since William James, courses in 'general psychology' are essentially 'courses in English.')

'Random' is defined in the dictionary as 'done at hazard, left to chance.' And in this sense it is used by psychologists, I regret to say. If its meaning had been altered by a scientific definition, there would be no complaint against that combination of six letters. But the present writer, who never uses this term in his own teaching, knows of no instance where one of those who use it has defined it. It is always used in the dictionary sense of chance action. And what nonsense is that!

[1] "Es erben sich Gesetz' und Rechte
"Wie eine ew'ge Krankheit fort—
"*Vernunft wird Unsinn*, Wohltat Plage."
Goethe.

How can we teach psychology as a science if we emphatically suggest to the student that in all the other natural sciences, but not in psychology, every cause has a quantitatively (that is, in space, time, and matter, centimeters, seconds, and grams) determined effect? If there is anything undetermined, 'left to chance,' in this world, it must seek its place in religion, in poetry, in metaphysics. In the natural sciences, including psychology, it has no business. 'Random actions' have no business in psychology teaching. The text-book writer who writes of them, the teacher who speaks of them before a college class, stultifies himself.

Thirty years ago there existed a school of psychologists who called themselves 'genetic psychologists.' Some of them are still alive. And there are still alive, in our normal schools especially, many of their disciples who know nothing of psychology but what each was taught by his 'master,' and who will go on teaching that until death calls them. For that reason a thousand courses in 'genetic psychology' are still being given every year as they were thirty years ago. Among these genetic psychologists 'random activity' is the chaos from which the 'creative willpower' with the help of 'pleasure' (others say 'satisfyingness') evolves the cosmos, the order of 'conscious and purposeful activity.' Nothing can be said, of course, against the poetic and literary fertility of these terms. He who does not use them throws away the material for the construction of a 'readable' text-book. (By the way, who ever heard of, or desired, a 'readable' algebra text-book?)

Psychology has a peculiar intermediate position between physiology and sociology. As science of human life, psychology is separated from physiology only by the fact that those phases of life activity which, like swallowing, intestinal action, blood circulation, are of virtually no (that is, relatively little) social significance, appeal mostly to the physiologist, whereas fighting with the pen or the sword is an example of what appeals to the psychologist. On the other hand, as the science of *the socially significant part of human life*, psychology is separated from sociology only by the fact that certain phases of life have a distinctly *historical* (but *not* using the word as

opposed to prehistorical) origin. They are 'institutions.' Not always legalized, but accepted as if they were, (think of intercollegiate football, more firmly established as an institution than many a legalized institution) they might be called customs when legal sanction is lacking. The psychologist leaves these 'institutional' phases of life to the sociologist. The psychologist is interested in 'fighting,' but not in the history of the duel; in social instincts, but not in totems.

Holding this intermediate position between physiology and sociology, the psychologist has to look out against confusing influences coming from these two sides. The genetic psychologists who talk of the 'random' activities of the young, are psychologists who have been unable to withstand the confusing influence coming from one of these sides. They have been captured by sociology, have unwittingly become sociologists. "Many learned acts develop out of the child's random activity. Out of the great variety of the random movements certain ones are picked out and fixed." Such is their teaching. By the way, this quotation is intentionally taken from a text printed very recently and being perhaps the most popular one of our very day, in order to show that the 'genetic' psychology is still with us, even between book covers where we do not expect it.

For the sociologist it is natural to be interested in human action. Will a man give a dollar for this charitable enterprise or will he kick out the collecting agent? That the man has all kinds of sense organs does not interest the sociologist much. It is as easy to write to him as to talk to him. Whether he 'does' this or that is the only difference that matters. So the genetic psychologist becomes fascinated by the 'awkwardness' of the little child's first writing and the elegance of his later penmanship. He gets the illusion that the difference consists mainly in the 'too great variety of random movements,' at first, and 'the few certain ones picked out,' afterwards. Being biased in his aim of observation, he fails to see that the *variety* of muscular activity is likely to be equally great and that this variety, anyway, does not matter in the case. He fails to see that what matters is the following fact. Of the great variety

of muscle contractions, all determined, the *strongest* one, at first, was an *inessential* one; and of the equally great variety of muscular contractions, afterwards, the *strongest* one was the *essential* one. The genetic psychologist has a wonderful aptitude for overlooking how a letter writer chews gum, sighs, wriggles his toes, pulls his diaphragm up and down, puts his knee joints under the most varying stresses, squeezes the fingers of his left hand, bites his lips, etc., etc. He overlooks all that because he is fascinated by the beautiful forms in which the ink flows from the pen over the paper. Nevertheless, the problem has nothing to do with any such nonsense as the assumption that muscular contractions in the young happen at random, 'at hazard, left to chance.' The muscular contractions of the young are as little 'random,' undetermined by cause, as those of the adult. If that were otherwise, psychology could be no science. He who teaches such nonsense commits a veritable crime on the young scholars who sit at his feet.

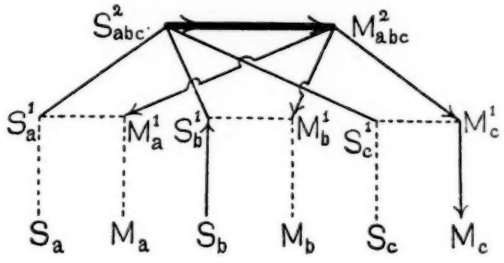
The genetic psychologist is so overawed by seeing that the thing he expects really happens in the *adult*, that he has no eyes for the other things which happen too, though less conspicuously. And he is so disappointed when the same beautiful thing cannot be seen in the *young*, that he makes a great ado about all the other things which happen,——as if, with a multitude of excited sensory points anything else could be expected than a multitude of muscles contracting, in either young or old.

The genetic psychologist, confining his peculiar interest to a single one of the many actual sensory excitations, through his own fault is intellectually swept off his feet by the multitude of childish responses and exclaims, "What an unexpected variety! Let us call them random responses!"

"My dear sir," let us speak to him thus, "the dictionary does not say that 'random' is synonymous with the peculiar combination of this adverb and this adjective, 'unexpectedly variegated,' with the further implication that the lack of expectation here is really the observer's own fault. And as a man of science you should further be aware of the fact that

'random' is a term utterly inadmissible in any science as one of its technical terms. But perhaps it is really the desire to produce salable 'literature' which made you write this 'scientific' text."

In order to keep in touch with the title of this article, let us see if we cannot suggest to the genetic psychologist a phrase which he has no reason to reject, and which for friendship's sake he will accept. He clearly is regarding the stimulation as identical in the two cases, that of the young and that of the adult. He means to convey that the stimulation which is relevant here, is—in simplified, schematic language—a stimulus applied to one sensory point of a nervous organism which has several ('random' equals 'variegated,' we remember)



motor points. Now let us use that diagram, simplified to extremity and yet complete, of such a nervously equipped organism which I have used for twenty years, with less consistency during the first of these years, with perfect consistency and definite lettering during the last fifteen. In this diagram we have three reflex paths, since we need for our present argument 'several' motor points. Two reflex paths, of course, would suffice but would look like stinginess on our part. Three reflex paths can have no fewer than three sensory points, or they would not be three 'paths.' In addition we have a 'higher center' which 'integrates' the three reflex paths. It is extremely important to notice that from any one of the three sensory points a nervous flux always goes (but not equally strong) to all the motor points, and that over the 'partial' path $S_{abc}{}^2$ to $M_{abc}{}^2$ the flux always goes (by postulate)

from the *S* point to the *M* point, never in the reverse order, from right to left.

Of course, 'a path' (that is, a whole path; if we mean a part of a path, we say so) always goes from a sensory point to a motor point and represents a sensori-motor function. No nervous flux ever originates in the brain, at least not normally. There may be rare instances, in insane asylums, of cases where an inflammatory disease 'starts' a nervous flux in the brain. While that is thinkable, it is not probable, even in disease. The conception of the brain as a tank of energy stored for distribution and released (by other energy than an equal amount flowing simultaneously inwards) is ridiculous because it presupposes that 'someone,' a mythological entity, 'thinks' the time has become ripe for opening the spigot. And equally ridiculous is the conception of an inward-going stream being 'dammed up,' because it presupposes that 'someone,' a mythological entity, 'thinks' it is wise to put, for the time being, a dam across the stream. Of course, those who are capable of conceiving of 'consciousness' as sitting in the brain and 'doing' material work, will disagree. For them 'consciousness' will attend to closing the dam and timing the spigot. Not all psychologists have yet freed themselves of mythology and metaphysics.

The phrase we want to suggest to the genetic psychologist is that of 'the initial common path.' The genetic psychologist confines his interest to cases of one sensory excitation and of a variety of (a 'randomness' of) motor effects. His three motor effects in our diagram are located at M_a, M_b, M_c. Three paths lead to these three motor points. They are the paths (let us toss up a coin to know which is the point of identical stimulation for the young and the adult!) $S_b\,M_a$, $S_b\,M_b$, $S_b\,M_c$. It goes without saying that there is a shunt, a true 'reflex path,' shorter than those enumerated, from S_b to M_b. But that shunt is of minor interest just now, and that is the reason why it is dotted in the figure. The three paths have a 'common initial path.' The phrase 'initial' is equivalent to 'near the sensory side' and virtually synonymous with the 'sensory' nerves of the text-book physiology. 'Final' would then of

course have to be equivalent to 'near the motor side' and virtually identical to the 'motor' nerves of text-book physiology. Surely the genetic psychologist, for friendship's sake, will adopt the term and, instead of talking of random movements, will say that it happens sometimes that a multiplicity of sensori-motor functions have 'an initial common path.' It happens in this case that 'the initial common path' has the name $S_b\ S_b{}^1\ S_{abc}{}^2\ M_{abc}{}^2$. Remember that it ends at $M_{abc}{}^2$, at the *right* of the heavy line of the diagram.

The genetic psychologist was always interested in 'the initial common path,' altho he did not know it because nobody ever suggested to him this happy phrase, whose patent rights belong to Sir Charles Sherrington, altho he has not hitherto known that either. The genetic psychologist was interested in the fact that *in spite* of 'the initial common path' (supposed to be single) the young animal reacts with one limb and the adult with another limb. This variation from youth to old age is 'learning' as the genetic psychologist sees 'learning.' What could be of more interest to the genetic psychologist than learning? Indeed, the 'how' of learning in the sense of 'how improvable is action' is synonymous with genetic psychology. From what we have said before it follows as a corollary that, if the *sociologist* is interested in any such thing as a 'common path' at all, he is interested in 'the initial common path.' Talking of 'random' activity is identical with unwittingly professing the doctrine that 'the initial common path' is the center of our bias.

Having disposed of the unfortunately so-called 'random' responses of the untrained, we have to take under advisement the second of our problems, the 'conditioning' of reflexes. If one would believe some of those who can write no page of 'psychology' without mentioning at least once the great discovery of 'the conditioned reflexes,' the fact that when a hungry tramp stops before the show window of a 'delicatessen' shop, 'his mouth waters' was entirely unknown to the world at large twenty-five years ago, and suspected by only a few geniuses. But if that were true, the phrase 'his mouth waters' could not be the common idiomatic expression it has

been for hundreds of years. The trouble with those who waxed so enthusiastic over the conditioned reflexes was the simple fact that they had not properly analyzed the various forms of learning, of habit formation. If they had done that, if they had reduced all habit formations to their simplest prototype, they would have realized that no habit can be formed as long as the sensori-motor function is unitary on the sensory side. Example: nobody sneezes better after having grown older and therefore sneezed oftener. No habit can be formed except under the 'condition' that a second sensori-motor function co-operates somehow with the given one. The second sensory excitation, of course, is 'the conditioning stimulus.' The 'conditioned reflexes' are nothing but a new title for an old story.

Nevertheless, the influence of the physiologists experimenting on conditioned reflexes has been a most desirable, an exceedingly healthy one, for it has been an additional broom to sweep the metaphysical tendencies from psychology. Let us abstain from belittling individual psychologists because they waxed enthusiastic over the conditioned reflexes. As the genetic psychologists were unwittingly captured by sociology on one frontier, so these psychologists on the other frontier were captured by physiology and became unwittingly physiologists. As physiologists they experimented, of course. Physiology is much more than sociology of the nature of an *experimental* science. Leave alone extirpation of nervous substance or of a muscle, how could the physiologists *experiment* except by varying the stimulation, by applying the stimulus now here, now there? But, whereas the sociologist observer has his eye on the *single stimulus* and the 'random' *responses*, the physiologist observer has his eye on the 'random' *stimuli* and the *single response*. It is no wonder, then, that in such a book as Sherrington's 'Integrative Action of the N. S.'[1] we run across the phrase, invented by him as suiting the physiologist, of 'the final common path.'

Of course, the word 'final' is but equivalent to 'near the

[1] In it just the opposite of 'integration,' that is, experimenting on the separate though mutually dependent functioning of several muscles, is the chapter of discussion. The 'integration' of muscular activity would be, if extreme, an epileptic fit!

motor side' and virtually synonymous with the 'motor' nerves of the text-book physiology. In our diagrammatic figure of a simple nervous system the path (let us toss up a coin to select a motor point) $S_{abc}^2\ M_{abc}^2\ M_c^1\ M_c$ is 'the final common path' in the physiologist's sense. Remember that it is only a *piece* of a path, for it does not go from a sensory point to a motor point, but starts in what is really an *interior* point of a *complete* path. Remember that it starts at S_{abc}^2, at the *left* of the heavy line of the diagram.

A concrete example of experimentation is easily given. S_c is a dog's back. M is the muscle moving the scratching leg. S_a is the ear to which a rasping noise is applied. M_a is a muscle; but the physiologist does not care which it is among the many. The back and the ear are stimulated with not too much intervening time, though not by necessity simultaneously. Ultimate result: when the rasping noise is the only stimulus, the leg nevertheless scratches. That is, by far the major part of the excitation caused (only) at S_a goes over $S_{abc}^2\ M_{abc}^2$ to M_c, very little to M_a. Since the physiologist personally cares nothing about M_a anyway, he mentions $S_{abc}^2\ M_{abc}^2\ M_c^1\ M_c$ as the 'final' (that is, motor) path and calls it 'common' because he *experimented* with *two* sensory points, S_a and S_c.

To the psychologist the term 'common path' is equally nonsensical in both the sociologist's and the physiologist's sense. The psychologist has no right to restrict his interest to one, either the sensory or the motor, phase of the sensori-motor function. To him the only use of the phrase 'common path' which is in accordance with common sense is the use which restricts this phrase to that nervous tissue which is *always common to any multiplicity* of sensori-motor functioning of the nervous system, that is, in our diagrammatic figure, to the heavy black line in the diagram, to $S_{abc}^2\ M_{abc}^2$, to the 'higher nerve center' if you wish to call it thus.

Nothing else deserves to be called 'common path.' The physiologist gets the illusion of a *final* common path only because he, during his 'conditioning' experiment, is not interested in and overlooks the response which reflexly belongs to

'the rasping noise.' The sociologist gets the illusion of an *initial* common path only because he, in his sociological observation, has without warrant taken it for granted that 'random' responses can result from a single in kind and identical stimulation and has not interested himself in, has overlooked, that the variegated response was the natural result of a naturally always variegated stimulation.

The psychologist is under obligation as representative of his own science to preserve a broader interest, to free himself from these illusions. To him there is no sense in a 'common path' when either 'initial' or 'final' is added. To him there is sense only in a 'common path' which is an interior piece, a 'nerve center' (generally deserving the name of a 'higher' nerve center) of the sensori-motor mechanism, $S_{abc}{}^{2}$ $M_{abc}{}^{2}$ in the diagram.

After reaching this conclusion and thesis we might stop. But it is perhaps worth while to point out that a clear conception of 'the common path' is more than an exercise in logic. Without clearness in one's conception of the common path *no understanding of the process of habit formation is possible.* The best, then, would be vague diagrams like those of Mr. Woodworth's[1] 'linkage,' of whose lines neither he nor anybody else can tell what they hypothetically represent in a *material* sense, as *anatomical* (non-metaphysical) entities. Or what conceivable material functions, as physiological possibilities, can be meant by 'drawing on an ineffective stimulus (sense organ?) and bringing a linkage into use,' as quoted from page 402? As a fellow teacher, the writer sympathizes with Mr. Woodworth. The temptation is strong to make our teaching over-simple and thereby palatable to not overstrong students, to sugarcoat it with figurative language like 'linkage,' and to *simplify* the diagrams until they *lose all* meaning, so that no student is exposed to the bitter experience of 'being unable to get their meaning.' Pitilessly, by the teacher, in any diagram illustrating the neural changes corresponding to habit formation, 'the common path' must be drawn. The intellectual weakness of our students is a poor excuse for omitting it from the drawing.

[1] 'Psychology,' Fig. 55, *ff*.

Is the present reader willing to accept the intellectual burden, and is he enough of a true psychologist defending his field against both the physiologist and the sociologist to be interested in 'the common path' in its universally correct meaning? He can find the significance of 'the common path' brought out if he will place before him page 124 of the writer's 'Psychology of the Other-One' and page 10 of his 'Psychology Demonstrations.' The former page contains the needed diagram, the latter a brief statement of the *two stages* through which every *learning* process passes.

To quote at length would be unjustifiable. But this much can be said here; that in the first stage of every learning process 'the common path' becomes more and more ascendant over the reflex paths. And in the second stage, if relative intensity of stimuli happens to suit this result, one of the motor ends becomes ascendant over the other motor ends with relation to one of the several stimuli of several sensory ends of the conductive system. The genetic psychologist rarely becomes interested in a habit forming individual until after the first stage has far advanced, and so he labors under the illusion that there exists such a *natural* state of affairs that an identical (simple) stimulation produces 'random' (multiple) effects. The 'genetic' or 'educational' or 'sociological' psychologist was unacquainted with that individual before that time (childhood, infancy, babyhood) when singleness of response to single stimulation became inconspicuous,—unacquainted with him at that time when singleness of response could still be conspicuous. So he falls under the mystic spell of 'random responses.'

The physiologist, on the other hand, is likely to be most interested in the rather primitive conditions of life and experiments applicable to them. So he is apt to talk of a *multiplicity of stimuli*, for one of which he has invented the special name 'conditioning stimulus'; but to talk of only *one final* common path, the one *on which he happens to have his eye* while experimenting. He is blind to the existence of the *other final* paths, equally common. And worse than everything else, he is blind to the logic of the plain truth that an *ultimately unitary* (but

habitual, acquired) *sensori-motor function* cannot be anything but *one ultimate motor* response belonging to *one ultimate sensory* excitation, in which case the term 'common' has lost all meaning.

To sum up: to the psychologist who keeps intact his much needed rôle as mediator between physiology and sociology, a 'common' path is neither initial nor finai, but in accordance with universally applied logic (if applied) is the intermediate tissue which bridges over the tissue making up two, three, or more reflex paths,—the heavy line of our diagram. And if he has the courage of his convictions he must draw that line,—and not leave it out and then talk of mythological figures like 'satisfyingness' or 'pleasure' or 'displeasure' coming in, armed with the cutting pliers and the soldering iron of the line man, and 'breaking connections' and 'making bonds and linkages.' Where there are no connections in the first instance, mythological figures will never make any.

PERSISTENT PROBLEMS IN SYSTEMATIC PSYCHOLOGY

III. STIMULUS-ERROR AND COMPLETE INTROSPECTION

BY RAYMOND H. WHEELER

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In the two preceding studies we discussed, first, psychology as the science of mental processes and concluded that this definition was a philosophical heritage based upon a violation of the structure-function logic; and second, the datum, our concept of which harks back to a dualistic position and to an unsuccessful treatment of the structure-function problem as it relates to subject and object. The concept of the 'given,' used systematically, leads to a dilemma, namely, that of forcing content, alone, upon a situation in nature which must inevitably be regarded from the functional as well as the structural viewpoint. In other words, the given, like the statistical formula, is barren without logic behind it and this logic pertains to the dynamic. Further, we pointed out that in practice we actually violate the dogma that the datum is content only, whether we are process-psychologists or not, and that this violation exceeds the mere expedient of calling the content a process. If we do not treat the datum—and to treat it is equivalent to defining it—as content-in-use, we have no science but a body of relatively meaningless and unorganized facts. Accordingly, the distinction between process and meaning is not only artificial but dangerous, except as a way of expressing different degrees of analysis and control of a situation. It is not a distinction upon which the definition of the science can rest.

In general the same considerations hold for the stimulus-error. In fact it would appear that a distinction between process and meaning, made in an effort to avoid the stimulus-

error leads the process-psychologist into the same dilemma which, supposedly, he was escaping.[1]

The issue before us is systematic, and is not concerned with the problem of unequivocal correlation, as such, between stimulus and response. As we shall attempt to show, this latter problem has nothing to do with the stimulus-error as a systematic concept in psychology.[2]

The object of science is to predict and control. Scientific description is a means to an end. Success in any scientific project demands that conditions be ascertained under which there is an unequivocal correlation between two or more events. If the event chosen as cause is not followed consistently by the event chosen as effect, it is certain that unknown and uncontrolled factors are intervening. The error, however, is never more than an incomplete analysis or control of the situation. It will not differentiate a physical from a chemical datum nor a chemical from a biological one.

[1] To prevent confusion it might be well to mention here that we are in entire accord with Boring's recent article on the stimulus-error (*Amer. J. Psychol.*, 1921, 32, 449–471). As the reader will no doubt conclude for himself, however, we differ, apparently, in that we would have carried the discussion still further by way of pointing out its value in bringing two opposed points of view more closely together. We regard Boring's paper as one of the best arguments on record for a psychology based on modified behavioristic principles. We differ also in our belief that the situation, as pictured by Boring, is an excellent argument for 'complete introspection' and against the 'intensive' or verbal report method ordinarily employed in the psychophysical experiment.

[2] Much confusion has arisen, in psychological discussion, owing to different uses of the terms, stimulus and response. If by stimulus we mean the external force which acts upon the sense organ and ends there and by response a discharge from the motor centers, together with the contraction of a muscle or secretion of a gland, we are thinking of the situation from an anatomical and structural viewpoint. If we mean by stimulus something relative, a stage in an interaction process, and by response a later stage in that same interaction process (here the term adjustment is better), we avoid the arbitrary distinction between the two demanded by the anatomical bias, and pave the way for a functional viewpoint. In this series of papers, the words, stimulus and response, are used merely as convenient descriptive terms with no intention of limiting either to the operation of a specified muscle or neuron or to an absolute position in space. An organization of the facts of behavior will be no more successful when the situation is thus made a mosaic than was associationism or faculty psychology. For this reason, we cannot feel that a stimulus and response psychology is incompatible with a psychology which seeks either to emphasize a 'central factor' or an 'organic urge.' (*Cf.* Thurstone, 'The Stimulus and Response Fallacy,' PSYCHOL. REV., 1923, 30, 354–369.)

The differentiation depends upon other factors entirely. In other words, a specific field of science is not defined by improper controls. Accordingly, the stimulus-error will not differentiate physics from psychology or introspective psychology from behavioristic psychology. Neither will it distinguish psychology from logic. A universal error will not define a particular situation, nor is the fact of equivocal correlation a substitute for epistemology.[1]

The source of error in psychological experimentation is the same, typically, as in any science. If we are inclined to force an epistemological issue on psychology, we must also burden all science with it. In continuing to speak of the stimulus-error, psychology has unsuccessfully divorced itself from epistemology. The double-aspect theory and the stimulus-error go hand in hand. The physicist commits the same type of error when he tells us that a piece of iron weighs two pounds!

Now let us seek the concrete facts which are supposed to support the stimulus-error. It has certainly not been our experience that the physicist, psychologist, and artist cannot

[1] The Machian epistemology is useful when it comes to the understanding of how it is that we can comprehend each other, but a dangerous position upon which to build a scientific system. To abide too closely to 'aspects' and 'phenomena' is to make interpretation of fact logically impossible, for interpretation is based upon the logical construct of the dynamic. To interpret at all consistently, we are forced to assume a dynamic datum.

We are in accord with Young's use of the word 'phenomenology' (PSYCHOL. REV., 1924, **31**, 288–296) in which he defines the datum as a set of relations, *i.e.* a content as a pattern, not a content as a 'stuff' in any animistic sense. But it would seem to us that his view of the pattern is too structural. The pattern derives its systematic advantages over the idea of content in that it is a 'photograph' so to speak, of a dynamic process. It is derived through function. Bentley (PSYCHOL. MONOG., 1921, **30**, 1–15) seems to fear dynamism because of its mystical implication. We believe that such an attitude is evidence that the problem is misunderstood. Dynamism *is* dangerous when it parades in *structural clothes*. Animism is an example. Reading contents as reals or entities (rather than 'forms' or 'modes') into any situation from a structural viewpoint is the reverse side of reading a *vis a tergo* into content or structure from a functional viewpoint. Either tendency violates the structure-function logic and leads to difficulties. When the dynamic is regarded merely as a logical construct, an unknown, to which almost any appropriate name could be given, such as energy, we escape rather than enter upon difficulties, provided we adhere consistently to the same logical position and do not inadvertently shift from one viewpoint to the other without recognizing what we have done. (*Cf.* Wheeler, *Monist*, 1923, **33**, 556–567.)

equally as well judge the relative saturation of two colors or the relative intensities of two tones without having recourse to a distinction between process and meaning. The introspectively-minded psychologist is no more accurate in his sensory discriminations than the woodsman, the tea-taster, the lumber inspector, or the deaf-mute who distinguishes people by smell. Rarely can an expert in the workaday world tell us how he makes his discriminations. Are the psychologist and the layman making the same discriminations? Either may make mistakes—judge crudely, by using wrong criteria. He simply misses his cue. Now the question is, will the distinction between process and meaning define that cue? When the layman, without recourse to a distinction between process and meaning, discovers the proper cue, and when the psychologist discovers his, are they necessarily different cues?

Introspective analysis—the method responsible for the distinction between process and meaning, because it has been applied to the problem superficially—may also be used as a source for the evidence that this distinction is artificial and that object-mindedness is not the source of error *except as the observer centers his attention upon the wrong object.*[1] Take the case of lifted weights, for example. Introspections do not show that the observer cannot train himself to judge adequately to the limit of human possibility by centering his attention upon the object or an appropriate feature thereof. Suppose that we find his judgments to be more uniform when he attends to 'strain-in-the-arm.' Even then the process-psychologist would not have a case, systematically, because 'strain-in-the-arm' is an object-consciousness. He has not assumed a process-attitude. Attending to 'strain-in-the-arm' is not the same as attending to the 'intensity-attribute of kinesthetic sensation.' The one is said to be meaning and the other, process. The first commits and the second avoids the stimulus-error.

In other words, the process-psychologist has yet to show that, in case of a correction following the observer's cognition

[1] *Cf.* Bentley, 'Field of Psychology,' 1924, p. 410*f.*

of 'process' the improvement was due intrinsically to a shift in attitude and that the stimulus-error as he defines it, has been avoided. On the contrary, the improvement has been due to a reliance upon a single rather than a multitude of variable cues and, as far as systematic psychology is concerned, it is a coincidence that the 'stimulus-error' had anything to do with it.

In spite of the fact that the psychology of perception points to the impossibility of finding any mental processes whatever that are preperceptual, process-psychology has persisted in its attempt to catalogue them. The demand is repeatedly made of observers to isolate such data, perceptually, and then these abstracted data are used, systematically, as something simpler than perceptions. This might be admissible if the process-psychologist did not claim that these abstractions possessed *observable* properties!

One is instructed, let us say, to describe a tone. He reports intensity, pitch, quality, and volume. He has been analyzing a complex tone-consciousness or perceptual pattern. This is as easily done as noting visual and verbal imagery in one's response to the word knife. In both cases percept or meaning has been resolved into its ingredients. But the abstracted processes are still percepts and are *still complex.* Now force the observer to ascribe any property whatever to the tone or to the tone-quality and he will commit the stimulus-error. Let him so much as pass the feeblest attributive judgment and the tone-consciousness becomes 'object.' This is easy to see. The tone-quality as such is an abstraction —an inference—and not in any genuine sense of the word an immediate experience. When the observer refers to tone as the thing qualified, it is not the abstraction—the non-existent —to which he is referring. It is the tone-consciousness or perceptual pattern—a meaning, as definite a thing as any meaning and as much object as one's awareness of a weight. It is not an auditory process which has been described; it is a meaning which has been stated!

For a brief instant the observer was analyzing and really describing. This was the process of resolving the perceptual

pattern, qualitatively. Then he began to supplement. This is all he could do. A quality consciousness is not the same as an intensity consciousness. Each is a distinct percept by itself. The best he could do was to describe one in terms of the other by slipping into the so-called meaning-attitude and treating the tone-consciousness *as if it were a 'simple'—a preperceptual process.* To perceive a tone intense is exactly as much a stimulus-error, systematically, as to perceive a whistle as shrill. It is in this same way that we observe our 'bright' and 'dull' tones, 'large' and 'small' tones, 'granular' pressures, 'filmy' colors and all the rest of the synesthetic family. There is no difference in principle between an 'intense' tone and a 'red' one so far as the psychology of their observation goes.

Tone-quality as process, be it remembered, is an abstraction. It is not known away from context. It is meaning. The context of which the so-called quality is a member is the feature which really furnishes the quality as perceived. Let the observer listen for quality and not intensity or any other factor in the original tone-consciousness and he is forced to supply a different context. The tone is forthwith placed in a visual, kinesthetic or other schema. Under these instructions it is as impossible for him to imagine what the quality is like, minus a visual or a kinesthetic associate as it is to imagine intensity without quality. Force him to abstract intensity from the tone-consciousness and intensity is perceived in terms of visual or kinesthetic factors. Not that the visual and kinesthetic factors are surrogates. The intensity *is* the context. The visual and kinesthetic factors are actually contents of the intensity-consciousness. Then, force him to pass judgment on that intensity and he will inevitably do it in terms of these associates. There is nothing else for him to do. The intensity consciousness or any other supposed 'simple' will not stand alone.

A specific quality-consciousness, likewise, is never pure quality. It is a tone-quality plus a visual or some other associate. When the observer begins to say anything whatever about the quality (or the intensity) it is *bona fide* evidence

that he is obeying the laws of perception by committing the stimulus-error. The only difference between the ordinary observer and the synesthetic individual in this respect is the fact that in the latter no amount of training and no degree of vigor of instructions can blind him to the so-called surrogates.[1] Usually, the associate is missed because the psychophysical experiment promotes the very meaning-attitude which it aims to avoid.[2] We are all just as cognitively deaf to tone-quality, for example, without a non-tonal associate as is the synesthetic observer in the absence of his color-associate. Let the trained introspector (trained in the complete introspective method) search his consciousness for the cues by means of which he characterizes a tone as mellow, round, smooth, rough, raspy, harsh, reedy, high, low, et cetera, and he will find these cues eventually. They do not pertain to the tone at all. They are stimulus-errors and pertain to visual, kinesthetic, tactual, and verbal 'fringes.' The subject has been stating meanings.

Although it sounds grotesque, when an observer perceives the word, Emma, in terms of pie-crust, or white as a white-sheet-of-paper, he is doing nothing more, systematically, than to perceive color as 'filmy' or a doubtful two-point consciousness as 'dumb-bell.' In the latter case, for instance, the pressure increment as such is neither dumb-bell or otherwise. It is a pressure-meaning and that is all. The dumb-bell experience is visual or kinesthetic and the dumb-bell pressure is an object-consciousness. Likewise, a filmy color is no more preperceptual than a red sheet of paper. The film is there in addition to the color, as object, and it can be detected introspectively.

We conclude, therefore, that there is no process-attitude

[1] The term, surrogate, is obviously a misnomer. The view that associates are extraneous and can be trained out of an observer by practice is unquestionably wrong. A tone simply cannot be judged in terms of itself. A visual, kinesthetic or some non-tonal process is necessary. Attempt to drive associations out the front door and others enter at the back. Training out one meaning-attitude is training another one in, unfortunately unknown to the observer in most instances because of his bias toward 'process.'

[2] *Cf.* Wheeler, R. H., 'Some Problems in Meaning,' *Amer. J. Psychol.*, 1923, 34, 193 *f.*

as opposed to meaning-attitude, except as we make process mean mental, and object mean something physical. Instructions may change the meaning, but they do not lead to an avoidance of meaning. Brentano's immanent objectivity is a process of interpreting inevitably found with any mental process and it will not be thrown out of court by a declaration that it is reflective and not descriptive psychology. Until psychology attacks this problem descriptively and solves it adequately, there will be no acceptable system. Awareness itself is a reflective process which is open, retrospectively, to observation.[1]

It follows, of course, that the unit of measurement in psychophysics is not a sense-distance.[2] In the first place, it is a stimulus-error to characterize the mental process involved as a sense-distance, because intensity as defined by the process-psychologist is an abstraction to which content is surreptitiously ascribed by way of a back door, and neglected. In reality it is an 'object-consciousness' having for contents, '*X*,' the parent-process,[3] elaborated to the cognitive level by associates such as visual and kinesthetic factors. In the second place, it is discussed in terms of the analogy of 'distance' or 'scale' which is evidence enough in itself to indicate that the sense-distance is a fiction. Even if it did have a criterion of its own, it would be uncommunicable and unknowable. In the language of the *Gestalt* psychology, it is the 'judgment of a difference.'[4] That it is a difference in intensity is a matter of interpretation and not of an immediate experience, called the 'given.'

We are not interested for the time being in mental measurement as such. Neither are we belittling its importance. But we venture the suggestion that since the true psycho-

[1] Apparently the difference between a visually synesthetic individual and a so-called asynesthetic individual is the fact that this interpretation shifts in the first case toward the visual increment and in the second, toward the auditory increment, in tone as an example. Each commits the stimulus-error. There could be no better argument for a functional psychology.

[2] Bentley, *op. cit.*, p. 410.

[3] Wheeler and Cutsforth, *op. cit.*, pp. 97 *ff.*

[4] Koffka, K., PSYCHOL. BULL., 1922, 19, 531–585.

physical judgment does not involve analysis but follows from it, and is an out-and-out statement of meaning, and since it follows at once upon the presentation of a stimulus and ends in a verbal response, it is a genuine behavioristic procedure. On the other hand, it is obvious that we would know a great deal more about the specific problems involved in the psychophysical experiment had the Bairdian and Külpean movement preceded Wundt and Fechner.

II

If our statement of the situation with respect to process and meaning and the stimulus-error be essentially correct, there can be no doubt that introspection ceases when a complex has been resolved into its ingredients. In other words, introspection is an analytical judgment, not an attributive one. It is always a statement of meaning, and it is differentiated from other kinds of observation only by the accessibility of the object and by the classes of meanings stated. It is essentially the type of introspection advocated by Külpe and Baird, a tradition that has recently been attacked as an impossible ideal.[1] Incidentally, the writer has been mentioned as possibly its only active exponent.[2]

One might anticipate that the foregoing pages would be evidence enough that complete introspection is the only intensive variety and that the verbal report method with its judgments upon single contents results in the reconstitution of processes which have been separated from their original contexts by the 'process'-attitude.[3]

[1] Pratt, C. C., 'The Present Status of Introspective Technique,' *J. Phil.*, 1924, **21**, 225–231.

[2] Boring, E. G., *J. Phil.*, 1924, **21**, 95 *f.*

[3] The process-attitude has been responsible for many a dilemma in introspective psychology. The psychophysical judgment can be analyzed, as Fernberger has shown. (PSYCHOL. MONOG., 1919, **26**, No. 6.) A judgment is a judgment anywhere. Ascribing different degrees of clearness to a mental process is exactly the same procedure as noting the relative sizes of two objects. The clearness is no more a property of the sensation than size is a property, technically, of the object estimated. It is a percept which is definitely analyzable. As a judgment on clearness it is useful, but it belongs to an entirely different phase of psychology than the process-psychologists generally assign to it. There is no systematic attribute of clearness, and the distinction between

The issue is so vital a one that we unhesitatingly challenge the assertion that the complete introspective method is merely a preliminary expedient to be used while training the psychological observer. It is to be admitted, frankly, that the method has been abused. The evidence is too well known to mention here. But this is not the point. There is a systematic principle involved which we cannot relinquish. We ask the process-psychologist to show that, in describing content in any circumstances, he is not making inadequate use of the method which he has attacked.

Pratt informs us that the complete introspective method is inaccurate because it does not allow for unequivocal correlation between stimulus and report. But, unequivocal correlation between stimulus and report, determined in advance, is incompatible with the scientific experiment. If there were no unknown and therefore no uncontrolled factors involved, the procedure would not constitute an experiment. Complete control and therefore an univocal determination is the situation found in the procedure of applying scientific facts after they have been ascertained. Pratt has left nothing for the experiment to achieve. The discovery of an unknown element or relation depends to some extent upon the ingenuity of the observer after his experiment is set up. Perhaps he has reason to anticipate what his discovery will be. He may have adjusted his controls accordingly. But in any given experiment, univocal determination is something to be ascertained, not something to be fixed in advance with absolute certainty. Even where verification is the aim, it is illogical to presuppose that a one-to-one correlation between control and the outcome has been determined beyond the question of a doubt. This view is apt to end in finding what one wants to find.

attributive and cognitive clearness is as artificial and misleading as its corollary, process and meaning. The numerous experiments which have been performed to prove the contrary have one and all committed the stimulus-error. (*Cf.* Dallenbach, K. M., J. EXPER. PSYCHOL., 1920, 3, 183–230.)

The psychology of affection is another example of the stimulus-error. Indeed the bulk of our conventional psychology of the simpler mental processes is, systematically speaking, a huge stimulus-error. Such is the penalty for so thorough a neglect of the complete introspective method.

On the contrary, the observer should be prepared to find not only what he expects, in accordance with his controls, but whatever else might happen as a result of partially known determination at the time. In the psychological experiment, where introspection is involved, this can be achieved only with a certain amount of freedom on the part of the observer. Give the least freedom, *i.e.* make it possible for him to introspect at all, and determination in advance has not been guaranteed by the experimenter. The observer's procedure may be directed along certain lines, and should be so directed. The ideal psychological experiment is one in which the range of the observer's memory-span will not be taxed. But it must be remembered that introspective memory is not rote memory and that reliance should be placed on training.

It is interesting in this connection that Pratt not only accepts as reliable a fact obtained by the complete introspective method, namely, the *Aufgabe*, but he uses this fact in characterizing the method as unscientific. To use the *Aufgabe* as evidence against the method, looks like the selection of data to prove one's point. While we agree with Pratt that uncontrolled mental sets may lead to unequivocal correlations between stimuli and reports, we cannot, in the circumstances, accept his particular argument as convincing.

Furthermore, Pratt's quotations from Baird's hectographed laboratory manual cannot be taken at their face value.[1] We do not know whether Baird intended these instructions to represent a systematic position or whether he dictated them to a stenographer on the spur of the moment, with the purpose in view merely of stimulating the students to maximum endeavor. The best we can do is to give Baird the benefit of the doubt and not accept too seriously Pratt's zealous emphasis upon the great detail with which the instructions were written. As a matter of fact the spirit represented in Pratt's critique is shared by all of us. The complete introspective method should, without question, be used under optimal conditions.

Again, in any psychophysical judgment the accuracy of

[1] *Op. cit.*, p. 226, 229.

complete introspections, previously given, is presupposed, otherwise we have no right to rely upon the validity of the process isolated and judged. If the preliminary work is not accurate—and Pratt tells us that it is not—why should our critic imply such a complacency of attitude toward the legitimacy of his isolated datum?

And finally, we shall do well to recall two important historical facts: The disintegration of the Würzburg and Clark groups who were working with the complete introspective procedure. Külpe's death, the war, and a misunderstanding of the *Beschreibung-Kundgabe* problem were undoubtedly responsible, in the main, for the defeat of the method in Germany. With Baird's long periods of illness, his untimely death, and the shift of interest, on the part of so many of his students, to applied fields, we can readily account for the lack of active exponents of the method in this country. We must take into account also the mad rush of so many psychologists into psychoanalysis and mental testing. That the complete introspective method has failed is to say the least a premature judgment.[1] On the contrary, when problems that are genuinely psychological are fully understood, the 'complete introspective method' will come into its own and the observer will be the anatomist of psychology. Indeed, it is doubtful whether Külpe or Baird realized the strength of their position, a strength which lies in the fact that *the simplest experience is complex.*

III

With structure-function problems straightened out and philosophical and epistemological problems laid aside, there should be little disagreement as to the rôle of introspection. First of all its only excuse for existence is the fact that mental processes are complex even in their simplest terms. Stated differently, we are all synesthetic. Second, the results of introspection are absolutely useless either psychologically or technologically unless they be regarded in the same light as the facts of anatomy. Their value lies in the furnishing

[1] Boring, *J. Phil.*, 1924, 21, p. 95 *f.*

of information on individual differences, and on such general principles as we need to know in order meaningfully to predict and control the behavior of the individual.

The psychologist who is interested only in facts of consciousness plays the rôle of the anatomist and no other. It is as illogical for him to define psychology as the study of consciousness as it is for the anatomist to define biology as the science of anatomical structures. We find ourselves in hearty agreement with Titchener when he tells us that there is no functional psychology of consciousness.[1] On the other hand, we submit the view that the rôle of introspection will be understood only when we hit upon an adequate functional psychology. We believe that the truly functional psychology is behavioristic in a modified sense of the term, whether we want to call it a psychology of capacity or not. This seems to be the only system which can handle the problems of dynamism, and there is no system which can avoid these problems and at the same time be complete.

Until we base our psychological theory upon that same neutral concept in the logic of science upon which all other scientific thought is based, we shall continue to have psychologies and not a psychology. The notion that psychology should not identify itself with scientific concepts in general is one vicious dogma and the view that the psychologist can give an adequate account of his datum exclusively in terms of physiology is another. Nevertheless psychology deals with the energies of the organism in so far as they are integrated in sufficient fashion to provide for behavior of a highly complex and variable nature. Introspection is a procedure enabling the psychologist to discover a part of the structural or organic setting in which this behavior takes place, but it does not result in a knowledge of the entire setting.

To recapitulate: We have seen that the stimulus-error resolves itself into a general scientific problem of control, on the one hand, and to an inevitable process of observing, on the other. It is an error only when conditions are not properly controlled or when the observer is suffering from an

[1] *Amer. J. Psychol.*, 1921, 32, 519–542.

illusion. It will not differentiate psychology from any other science or the true from the false psychology.

We have pointed out that what goes by the name of intensive introspection is not introspection at all but the reconstitution of a mental process under instructions which the observer is unable to fulfill. This reconstitution turns out to be the substitution of one meaning for another which the observer attempted to avoid. Throughout, his judgments depend upon processes which have erroneously been called surrogates. We have submitted the view that anything but complete introspection, under these conditions, violates the basic principles of scientific research, vitiating the observation where true analysis is desired.

In this and the two preceding papers we have suggested that certain problems raised by the process-psychologist are fictitious and keep subjective and objective psychology apart; that there will be no genuine psychology until the subjective and objective viewpoints are shorn of their differences; that the basic principles and naïve postulates for psychology are those for science in general. This makes psychology essentially a study of behavior in so far as that behavior is the product, in part, of consciousness, defined as a reconstituted stimulus.[1] The interpretations of which functional psychology consists can then be stated without fear of animism or other dangers of dynamism, and the structuralist, so-called, will still have his work to do.

[1] Bode, B. H., Psychol. Rev., 1914, 21, 46 *ff.*

CONTRIBUTIONS TO THE PSYCHOLOGY OF NUTRITION

III. NUTRITION AND THE FAMILY

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McDougall, in discussing the nature and origins of parental tenderness, calls attention to the remarkable difference between the feeling of the child for the parent, and the parent for the child, paradoxical in that the latter is likely to be much the stronger and more lasting of the two, although all the material debt is on the side of the child.[1] It is evident that here we have a crucial case in terms of which we may hope to try out our basic psychological principles. If we are unable convincingly to explain a matter so obviously important, so obviously arising out of the deepest essentials of human nature, then it would be best to consider a revision of our categories. The psychology of the family, in fact, is a part of the indispensable basis of any worth-while science of human nature, inasmuch as the family is the crucible of the human spirit, so that nothing which concerns it can fail to be of importance.

The Freudian treatment of the entire constellation of family emotions and reactions, a treatment which deserves notice because it at least represents a very boldly constructive and suggestive attack upon basic problems, would reduce all the manifestations of family life to a common foundation of sex. This would apply equally to the child's attachment to its parents, and the parents' attachment to the child, equally to the parents' selection of a favorite child, and to the child's selection of a favorite parent. All the crises of family history receive a specifically sexual interpretation. Birth, death, nursery competitions and attachments, the various antagon-

[1] McDougall, W., 'Social Psychology,' p. 73.

isms, submissions, triumphs, and special partisanships of the family drama, fears and scoldings, concessions and spoilings, are regarded as variants upon the single theme of sex. Upon the same cause depends the special psychology of the eldest, the youngest, or the only child. And by far the most crucial piece of educational work involved in the family relationship is claimed to be that of sexual enlightenment. It is certainly not to be denied that sexual interests do profoundly condition relations as between parents and children, and brothers and sisters. Above all, it would seem that the maternal concern for the suckling child has very distinct elements of sexuality about it, for in the process of nursing there is involved a constant stimulation of the most important of all the erogenous zones. Furthermore, it seems believable enough that the child's feeling for his parents may acquire a sexual tinge as he develops, since sexuality is a factor which tends to permeate the whole of life. But it does not seem warrantable to make psychosexual factors the basis of the social superstructure of the family. The view here to be put forward is that the family rests ultimately upon the foundation of nutrition, that the relationship of parent to child is in the last analysis that of feeder to fed. An attempt will be made to show that all the crises, all the differences in feeling and behavior, all the tensions that the Freudians have unearthed and attributed to sex are in fact trophic in their origin and nature, and that sexual factors are merely superimposed to the extent of modifying the situation more or less.

An attempt has been made elsewhere to show that there are insuperable difficulties in the way of saying, as the physiologists in effect do, that the taking of food is determined by two relatively independent factors, hunger and appetite. The process of feeding depends on nothing so simple as the tendency of the empty stomach to be thrown spontaneously into contractions of increasing violence, or the setting up of conditioned secretory reflexes.[1] Rather we have to do with a prolonged sequence of trophic education, which, in barest

[1] Mursell, J. L., 'Contributions to the Psychology of Nutrition: I. Hunger and Appetite.'

outline, turns upon the multiplication of oral experiences due to the persistence and universality of infantile sucking, which in turn, at last apprises the individual that the only adequate means of quieting hunger pangs is to swallow food, and the whole course of which is guided by positive chemotropisms for specific food substances, which coöperating with the conventions for which they provide a basis, tend to standardize the human ration everywhere and at all times. The remarkable rapidity with which this primitive piece of learning is carried on, and the high degree of permanence in the results obtained, is attested by a wealth of experimentation. Pawlow,[1] in his classic researches, has demonstrated the high sensitivity of the gastric and salivary reflexes to food signs of many kinds. A range of stimuli so diverse as the sound of a bell or whistle, the odor of camphor, temperature stimuli, colors, and so on, can be made effective as substitutes. With dogs as subjects, ten to one hundred repetitions were required to produce conditioning, the number varying with the nature of the new stimulus, and the success of the general set of the experiment. Once the stimulus had been learned, the new food sign became fixed with a high degree of permanence, being maintained for from two to nine months without any further training. Again, Boldireff finds it possible to condition salivation to the scratching of one particular area of the skin, and that the scratching of another will have no effect. Such localization, however, does not apply with thermal stimulation, which is differentiated in terms of quality rather than of position. Slight tone differences, too, are found to register very clearly with dogs, though this power of discrimination varies with the intelligence of the animals. Krasnagorski has determined that a child of six gives a differential salivary response to every tone of the scale. And Turro has shown that very young infants will speedily learn to respond positively or negatively to the colors of the clothes of their attendants according as they are or are not fed satisfactorily.

Clearly then, the taking of food is not determined auto-

[1] *Cf.* (*e.g.*), Pawlow, I. P., 'New Researches on Conditioned Reflexes,' *Science*, 1923, N. S., **58**, p. 359.

matically by any simple physiological mechanisms as, for instance, the intake of oxygen by way of the lungs is determined. Rather it depends upon a highly complex piece of learning, by means of which many elements originally disparate are brought together, and which may, and in fact does, work out very differently in different individuals. And the significance of this conception clearly is that here we are not dealing with a merely vital process, but with a type of behavior which very definitely involves the entire organism as a coöperating whole, and which, both for this reason, and because it constitutes the very first piece of learning ever performed, ought to be regarded as one of the most fundamental of all psychological interests. The average textbook of psychology, when it refers to these matters at all, is usually content to summarize the most important physiological findings regarding hunger and appetite. But this is a failure to get even as far as classic Greek thought, which did at least recognize an appetitive soul as part of the make-up of human nature. The viewpoint here defended is that the trophic interest, far from being one of the *residua* of human nature psychologically considered, is in fact one of its most basic elements. Trophic behavior is the first integrated behavior to emerge in postnatal life. For some years it remains the central point of organization for the entire personality. Little by little other interests, notably that of sex, expand to compete with, but never to overshadow, it. But it is the dominant note of the formative years, and very largely makes us what we are. And above all it lies at the root of our most crucial social relationship, the tie that binds us to our parents, and that indirectly determines our standing with the entire family group.

In referring to the suggestions of the Freudians, it was said that we would endeavor to show that the crises and tensions in family life, of which they make so much, were all to be interpreted in terms of trophic interests. We are now to enumerate certain relationships and events which are of determining importance, both immediately in regard to the development of family attachments and repulsions, and for

the future development of the child, and to point out how all of them come back to the nutritional interest.

1. *The Relationship of Mother to Child and Child to Mother.* —This, of course, is the vital nerve of the entire affair. From it everything derives. And here we have the most striking of all pieces of evidence of how essential nutritional behavior can be, and how central a part it plays in the integration and up-building of personality, and in the determination of social relationships.

On the mother's side, her primary relationship to her new-born infant is determined by the immediate necessity of feeding it. And this has far more than a merely morphological significance. The suckling of a child is a business which involves a woman's whole body and soul. Lactation is the natural consequence of pregnancy, and its healthy functioning tends to rectify and restore temporary endocrine imbalances, and to stabilize a personality somewhat deranged by the internal and external strains of gestation. McDougall vigorously protests against an older and cruder formulation of the same or a closely analogous idea. "Bain taught that it (parental tenderness) is generated in the individual by the frequent repetitions of the intense pleasure of contact with the young, though why this contact should be so highly pleasurable he did not explain. . . . This doctrine is a gross libel on human nature, which is not so far inferior to animal nature in this respect as Bain's words imply." [1] In spite of this edifying indignation, however, we must insist that Bain was substantially right. It is true that he failed to explain why maternal contacts with the young are pleasurable, but this was only because the reasons were not clear in his day. They have in general already been stated. Lactation and nursing, through their influence upon the endocrine system, are of the utmost importance for health and general organic tonus, as the sequelæ of pregnancy. It is not merely that nursing gives the mother pleasure. In fact it may or may not do so. The point is that, under at all favorable circumstances, it is a business very propitious in every way

[1] McDougall, W., *op. cit.*

to her best physical health and mental and emotional balance. For the mother, nursing is unquestionably to some extent a sexual performance, and to this degree the Freudians are right in their interpretations of intimate and primitive family relationships. But it is so complex and so essentially involves the entire personality, that to label it sexual is largely misleading. To sum the matter up, the child constitutes the health, the wholeness, the completeness of the mother, and it is hard to deny that here we have the primary basis for the great maternal emotion.

If the relationship of the mother to the child centers inevitably about the trophic experience, this is even more self-evident when we turn to the relationship of the child to the mother. Primarily the mother is to the child the source of food supplies. Thus it is not without significance that the young child will very often at first apply the same term ('mamma' or what not) to its mother and its bottle. The mother, under the pressure of a convention and also in response to her own physiological needs, assists in many ways the original trial and error process by which her child learns that eating is the way to still the pangs of hunger. Trophic learning is, as we have seen, very rapid in any case, and the essentially tuitional part played by the mother speeds it up still more. But this is by no means the whole story. We have remarked already that there appear to be positive chemotropisms for certain substances—proteins, carbohydrates, mineral salts, etc.—and the evidence for this view has been presented elsewhere.[1] Thus we may well assume that for the infant the mother's milk is precisely the satisfying substance that the organism requires. More goes on when the baby is fed than the inhibition of the contractions of the empty stomach. Definite and disturbing chemical needs are being satisfied, and a state of general well-being is in process of production. Furthermore, we must remember the very large number of stimuli which conditioned reflex experimentation has shown can become used as food signs; and surely when we are dealing with the repeated intimacies of family life, so

[1] Mursell, J. L., *op. cit.*

vastly more favorable in every way for the forcing of learning than the best possible laboratory conditions, the range of possible food signs must be very greatly extended. Hence, for the infant, the personality of the mother must be constituted of a complex of food signs, daily increasing in elaboration and subtlety, daily acquiring more and more power to guide behavior. The cadences and tones of the voice, bodily contacts, tricks and habits of manipulation, odors of various kinds, the colors and textures of clothing, all these and many more come to spell, not merely the elimination of uncomfortable stomachic sensations, but the highest satisfaction, the greatest euphoria that life holds. This is the process by which are forged the original love bonds which unite the child to its mother in its first and fateful social contact.

Additional testimony to the fact that nutritional elements are basic in the child's relationship to its mother, may be found in the well-known fact that breast-fed children tend to be physically superior and better advanced than others. Now theoretically it may be possible to feed to the child the precise balance of substances which are found in the mother's milk, even though this may be seldom or never actually achieved. In any case it is possible to make a very close approximation, so that the superiority of breast-feeding cannot wholly be ascribed to chemical differences in the ration itself. Even if such differences were entirely eliminated, the superiority would almost certainly still be found. And the reason is simply that the whole situation of breast-feeding, with its factors of comfort, warmth, and general satisfaction, is decidedly more conducive to proper digestion than the much more meager setting of bottle-feeding. If the personality of the young child means the mother's health, the personality of the mother also means the child's health.

We have already remarked that such an analysis as the foregoing seems preferable to that presented by the Freudian school, which regards this attachment as primarily sexual. It is able to maintain such a position largely by virtue of its explicit identification of all organic pleasure with sexuality. For us, however, the tie which binds mother and child together

is something far more basic than pleasure. It is, indeed, a relationship only one degree looser than that which obtains while the child is still in the womb, an almost organic connection by way of the trophic responses. And if this formulation is more plausible than one in terms of sex, it also seems decidedly better than the assumption of a filial instinct which unites the child to its mother. As one of the many and quite fatal difficulties in the way of such an assumption, let us consider the case of the orphaned or neglected child. Such a child will strongly tend to set up substitute attachments, to put other persons in the place of the lost parent. But these attachments are not random, nor directed towards anybody indifferently, as might be expected if they were the expressions of a generalized innate need to be loved or mothered. They are characteristically directed towards the nurse, who in fact usually stands second to the mother in the regard of the more happily situated child; and the reason is that she is the person who supplies food, doing so with the kindness and consideration requisite to furnish a favorable situation. The only reason why these substitute attachments are unstable and ephemeral is that they are not backed up by convention or enforced by law, and so are easily open to rupture at the will or whim of either party.

This view of the connection between mother and child being trophic in its foundations brings us to an explanation of what is probably the most important point in the interpretation of family psychology. For we must assuredly insist that the child's relationship to its mother is very far from being disinterested, still less, one analogous to an attachment between equals. It is, of course, of an utterly different character, and made of different materials, from the mother's relationship to the child. The original filial attachment is primarily and essentially one of the extremest organic dependence. It is not at first either love, liking, disliking, or hate; it merely spells the crying need of trophic satisfaction, and may be transmuted by very subtle influences into any one of the innumerable shades of attraction or repulsion. Reliable clinical literature, as well as many recent child

studies, most impressively suggest the crucial importance for later life of a wise regulation of the primary and powerful maternal attachment. It cannot be doubted that an injudicious exploitation of this attachment may easily do irreparable harm. Unwise coddling and 'spoiling,' as well as unwise coldness and indifference, often give rise to minor forms of nervousness, and may pave the way for the development of a neurosis. The influence of the parent upon later life appears in myriad subtle forms, and constitutes one of the great facts of psychogenesis. And at root it depends upon the factor of nutrition.

2. *The Place of the Father in the Life of the Child.*—The function of motherhood is pretty much the same everywhere and at all times. It remains substantially the same for the entire mammalian group. But the relationship of the father towards the children varies enormously in different types of social organization. It is, in fact, directly contingent upon his relationship towards the mother of his children. Here again we must protest against the importation of a parental instinct. The incidence of such an instinct is often defended on the ground that everyone, including savages, seems more or less 'fond of children.' But what we are dealing with is no abstract and generalized fondness for children, which is itself a complex factor calling for careful analysis, but an attitude on the part of the man towards his own children, which is inevitably reflected in their attitude towards him. And this depends more upon the form of marriage involved than upon any other single factor. With us, the status of the father in the life of the child is definitely overshadowed by the fact of monogamy.

Once more, however, we must refuse to entertain the sexual interpretation of the psychic phenomena of paternity. According to the orthodox Freudian view, the child is thrown into a special antagonism to the parent of the same sex, through jealousy arising from the desire for complete sexual possession of the parent of opposite sex. Thus a boy is said characteristically to develop repulsions towards his father on account of jealousy due to alleged incestuous impulses towards

his mother, which of course, for the sake of the most ordinary plausibility, are said to be 'unconscious.' There are numerous objections to this celebrated theory, but for the moment all that concerns us is that it entirely fails to fit the most patent fact of family life, which is that there is a decided preponderance of affection for the mother on the part of little children, quite apart from sex differences.

The reason for this becomes at once apparent if we bear in mind that family unity depends first and foremost on the satisfaction of trophic needs. For this satisfaction the mother is the primary agent. And the fate of the father, as an object of love, indifference, fear, or hate is determined first and foremost by the sort of relationship in which he stands to the mother in the mind of the child. Within the range of monogamous institutions, enormous differences are to be found in this respect, and granted monogamy, the connection between the mother and the father is almost certain to be close enough to be psychologically very important. If the father is closely and positively associated with the mother, above all if he is an intimate partner in the great business of nutritional education, then there is a fair prospect that the child will extend to him the regard with which he favors her. Of course it may be that later on sexual or other factors will intervene to shatter the forming ties, but at least a start will be made and the way will lie open. If on the other hand the father is entirely dissociated from the mother, or if he is found at all regularly to intervene between the child and her, then a beginning has been made towards an attitude of dislike and repulsion. In such an instance it is not hard to see how the tendency, once begun, will reinforce and be reinforced by all influences setting in the same direction, until finally, we may have some sort of neurotic condition which a psychoanalyst would diagnose as due to the 'Oedipus complex.'

3. *Fraternal Relationships.*—Once we grant the trophic character of the attachment between mother and child, the view that the child regards its brothers and sisters as objects of sexual jealousy seems untenable. No doubt such jealousy actually exists. No doubt, too, the only child, the youngest

child, the eldest child, and the child midway in the family, all tend towards a certain set psychological type, predetermined by their family relationships. But to explain all this it is not at all necessary to assume the incidence of sexual factors in any ultimate or decisive manner. The relationship of the child to its brothers and sisters, like that which it sustains to its father, is in the first instance a derivative of its relationship to its mother. Where there is but one child in the family this latter attachment is simple enough. But as the brood begins to grow in numbers complications arise, and all the problems peculiar to social groups manifest themselves in a form that is not the less intense for the smallness of the arena and the youth of the principal players in the drama. The drive for food, with its deep-going affective education centering on the person of the mother, is a motive quite powerful enough to account for the bitterest resentment being aroused by any object which makes its direct fulfillment at all doubtful or dubious. The natural attitude of the child to its mother is exceedingly possessive, with a possessiveness based on its extreme personal dependence. And the emergence of younger and favored competitors for the beloved person is naturally apt to cause an intense and quite complicated distress. Thus again we see how far-reaching is the nutritional motive, and how much of human life comes directly or indirectly under its sway, for we well know how antagonisms set up in the nursery may persist far along the years and produce profound modifications of behavior.

4. *The Significance of Weaning.*—Weaning as an event in psychic history seems to be of decisive significance in two directions. It involves a relatively high degree of separation from the mother; and it involves a projection into a depersonalized society.

It is the separation from the mother rather than the transition of solid food that seems to constitute the real difficulty in many cases of weaning. Sometimes when weaning has been unduly postponed, whether from unavoidable causes or not, we find the child manifesting an extraordinary stubbornness and sulkiness, which may almost go to the pitch of hunger

striking, which cannot be attributed merely to the slight inconvenience of forming new food habits. Much the same thing is likely to happen in a smaller way when weaning, though undertaken at the normal time, is made unwisely abrupt, and is not led into by some such devise as a gradual increase in the ration of bottle feeding and a slow introduction of a solid dietary. The only explanation of the child's highly 'unreasonable' attitude is that the parents have allowed an excessive fixation upon the mother to accumulate, and that an abrupt and ill-considered attack upon it constitutes a psychological outrage which is instantly resented. For it is surely very clear that the barriers are psychological rather than physiological. Thus weaning is to be interpreted as the first great step which the child takes on its own account. It is of absorbing interest to note how food and feeding habits easily become symbols of independence from the trammels of babyhood and nursing. The symbolic cambric tea, the appearance at the more formal meals, the eating from one's own plate of tastes of previously forbidden foods, the abandonment of the high chair, these and many other practices are treasured as pregnant with meaning of escape from the overwhelming dominance of the mother. Food deprivations, too, are perhaps the bitterest of all childish punishments, absolutely apart from any effect they might possibly have in producing hunger, for to the little child food is the parental gift *par excellence*, and its withdrawal a symbol of the refusal of parental favor. But the escape from tutelage is not altogether a matter of joy. Though the child ardently clings to the symbols of liberty and independent personality, he is pathetically reluctant to forego the substance of maternal care and oversight, and like most human beings, wants both to have his cake and eat it. He easily tends to assume a sort of sham helplessness—his plate must be specially served, his food cut up carefully, even though he is quite able to do this for himself, and various preparations for the meal must be made for, rather than by, him. By such means he continues to enlist maternal care.

We should note that all this means the evolution of a

new attitude towards the mother, which, however, is not in any sense a clean break with the past, but a development, for good or evil, of the older primitive attachment and dependence. Trophic education begins with the closest possible assimilation of the child to the mother, but it progresses by introducing barriers and distances between them. This is why the parental attitude towards the child is likely to remain closer and more affectionate than the child's attitude towards the parent. The mother has no such crucial piece of psychophysical evolution to accomplish as that involved for the child in weaning. For her, he is always the baby she nursed, while for him she becomes the being he managed to escape by way of a sort of second birth. Here then we have in part the explanation of the paradox noted by McDougall, to which we referred in the opening paragraph—namely, that in spite of the debt of gratitude being the other way, the parent is apt to be fonder of the grown-up child than he is of the parent. Of course weaning does not tell the whole story. In particular, excretory training, that is the education of the anal and urethral sphincters, and also complex sexual factors enter most importantly into the situation. But weaning marks the momentous beginning of a cleavage between child and parent, and no tact can be too careful to make such a transition propitious. We may strongly suspect, though without direct evidence, that the attitude taken by the adolescent youth to his parents is largely the result of the success with which the psychic transitions of weaning are mediated. Clearly, weaning should mean for the child a new birth of freedom without sacrificing what is valuable, and may be permanent in the more primitive and closer bond.

Again, weaning means the projection of the child into a depersonalized society, that is, into a nexus of rights and duties, and conventionalized requirements more or less arbitrarily imposed from without. So far, the act of feeding has hardly been social in any significant sense. Underlying all 'personality factors' there is the dominant physical relationship which tends to obscure everything else. It is only when the child begins to partake of solid food at the family table

that he comes for the first time into contact with an important body of positive convention, for here it is essential to do certain things, and to avoid others, and a code of 'proper' behavior is imposed, involving some degree of reciprocal consideration. The great psychic significance of weaning as involving a share in a common meal is interestingly and suggestively attested by the fact that in all societies the meal is a social focus which carries very deep and intimate meanings. It is surely not by any accident that the business of eating has accumulated about it a vast body of lore. Such matters as ceremonial purifications and prayers before eating; ideas of sacred amity in connection with the sharing of tobacco, salt, or bread; the preference for certain foods, especially meats, as containing special and mystical 'strengthening' principles; the religious consecration of foods; the sacramental meals well typified by the communion supper of Christendom; all these seem clearly to bear witness to a peculiar affective value attaching to a common meal. Thus when the child is transferred to solid food, partaken at the family table, it is a sort of first communion into a fuller personal and social status of being. He enters upon one of the most universal and profound of racial experiences, and the event is a peculiarly propitious and significant induction into the very heart of those primary or face-to-face associations which are in such large measure the school of human nature.

Having seen how the affective relations of the business of nutrition are interwoven with the life of the family group, and in fact constitute some of the most important phenomena of that life, we may now seek to sum up our whole discussion.

1. Physiological analysis, which regards trophic behavior as being conditioned by gastric hunger and appetite, seriously obscures the far reaching psychological importance of nutrition. Nutrition is a factor in human life as pervasive and as significant as sex, and to fail to recognize it is to render forever impossible any accurate dynamic appreciation of human nature.

2. As one of the relatively neglected aspects of nutritional psychology, we have presented a consideration of the significance of infantile sucking as a determiner of adult behavior.

3. The psychoanalytic school cannot be said wholly to ignore the importance of trophic behavior, but it very greatly exaggerates the importance of sex and underestimates that of nutrition. Furthermore, much psychoanalytic material dealing with the latter subject is fantastic and valueless. It has been the purpose of the last paper to show that many of the attractions and antagonisms which the psychoanalyst finds in the drama of family life, and which he attributes to sexual sources, are in the last analysis nutritional. It is not, of course, to be understood that any claim is made that the whole scope of complex and developed family life is trophically determined, but a study of psychological origins cannot fail to be interesting and significant if not pushed beyond the bounds of reason.

4. Finally it should be noted that in these three papers the entire subject is by no manner of means covered. Nutrition involves far more than the taking of food. Essential parts of the process are the assimilation of food by the tissues, and the excretion of waste products, each of which topics calls for a chapter to itself before we can begin to claim completeness for a study in the psychology of nutrition.

THE PHENOMENA OF ORGANIC SET[1]

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In the following paper I will present (1) a series of examples of organic set beginning with the simpler types and passing to the more complex, and (2) a few general statements based upon these illustrations.

I

(*a*) The smooth muscle of the mollusc *Pecten* is able to hold the shells in a closed or partly closed position. If a solid object be placed between the open shells, ordinarily they close firmly upon it and lock. If the solid object be removed, the shells remain for many hours in the partially opened position. The shells of this bivalve can support a weight for hours without the signs of fatigue which are usually observed in the case of skeletal muscles. It looks as though its smooth muscle contained some sort of a catch or ratchet mechanism[2] capable of holding the shells in a closed or opened position.

This mechanism is probably quite different from the mechanisms involved in the following illustrations of organic set.

(*b*) The work of Sherrington[3] and others upon decerebrate animals has shown that the entire musculature of the organism may be involved in maintaining bodily postures. A decere-

[1] A paper read before the American Psychological Association, December 29, 1924, Washington, D. C.

[2] Bayliss, W. M., 'Principles of General Physiology,' 1918, 533–548. The above statement is based upon Bayliss's discussion of the tonus of smooth muscle; Bayliss refers to the work of von Uexküll. There is good evidence that smooth muscle is capable of maintaining different postures independently of neural stimulation.

[3] Sherrington, C. S., 'On Plastic Tonus and Proprioceptive Reflexes,' *Quar. J. Exper. Physiol.*, 1909, **2**, 109–156. 'The Integrative Action of the Nervous System,' 1911. 'Postural Activity of Nerve and Muscle,' *Brain*, 1915, **38**, 191–234. Magnus, R., and de Kleijn, A., 'Die Abhängigkeit des Tonus der Extremitätenmuskeln von der Kopfstellung,' *Arch. f. d. ges. Physiol.*, 1912, **145**, 455–548.

brate cat is able to stand reflexly. If, while standing, the cat's head be forcibly flexed, the entire attitude is that of a cat looking *under* a shelf. If the head be raised, the animal assumes an attitude of looking *up to* a shelf.[1]

(*c*) A cat living in normal surroundings may sometimes be observed to crouch upon the floor as if prepared to spring upon a mouse. The cat remains temporarily motionless save for slight shifts of position, quivering muscles, and eye movements which follow some moving object. The entire neuromuscular machinery of the animal appears to be set for action.

Darwin [2] has printed the picture of a small dog in a bodily attitude of expectant attention. The dog is poised with the front leg slightly lifted, the head tilted to one side, and the ears raised as if the whole organism were observing an object and were ready to move. Bodily attitudes of this kind are maintained temporarily in the constant onflow of animal behavior. They may be readily observed.

(*d*) A further illustration of what is meant by 'organic set' is taken from Hunter's [3] study of the delayed reaction. Hunter found that the reactions of a rat in his discrimination-box depended upon the bodily orientation of the animal with reference to the position of the light. When the rat was released from confinement he moved forward in a direction determined by his bodily orientation. With dogs the determining cue was the direction of the head rather than the orientation of the total body. The raccoons, however, appeared to be less dependent than the other animals upon gross motor attitudes. They sometimes went to the correct food-box despite a wrong orientation at the moment of release and they sometimes went to the wrong food-box despite a correct orientation. In a word, the behavior of raccoons

[1] When a decerebrate animal assumes some bodily posture such as sitting or standing, the individual muscles change their length; but although the length of a particular muscle may vary, its degree of tension is approximately constant at various lengths (Sherrington). The relation between muscular tonus and bodily posture is very close.

[2] Darwin, C., 'The Expression of the Emotions in Man and Animals,' London, 1872, 43.

[3] Hunter, W. S., 'The Delayed Reaction in Animals and Children,' *Behavior Monog.*, 1913, 2 (No. 1), pp. 1–86.

appeared to be determined by intraorganic conditions of a sort not immediately manifest in the bodily posture. We may assume in this case that the organic set is purely neural, since the skeletal muscles give no clear indication of its presence. Hunter also found that children make delayed reactions in a manner similar to the raccoons.

(*e*) With man it is well known that a spoken word may call forth some bodily attitude. At the word 'ready' the runner toes the line, crouches, remains fairly quiet awaiting the sound of the starting gun. His neuromuscular machine is postured. An integrated neuromuscular pattern is temporarily established. His body is *set* for the race, as we ordinarily say. Consider also the prize fighter in the ring. He temporarily assumes an attitude of attack. His hands are clenched and his total organism is postured, as if prepared for action. The attitude of the runner, set up by a spoken word,[1] and the attitude of the prize fighter, established under non-verbal conditions, are alike in that they involve the same sort of neuromuscular mechanism.

(*f*) In the Stanford [2] revision of the Binet tests in year V there is a test of three commissions. The instruction reads in part as follows: ". . . Here's a key. I want you to put it on that chair over there; then I want you to shut (or open) that door, and then bring me the box which you see over there . . . ." If the instruction is understood, accepted and carried out, the child passes the test. While the child is receiving the instruction he may look at the objects mentioned and possibly start towards them. Normally, he is restrained until the entire instruction has been given and then he attempts to carry it out. Before the child begins to carry out the three commissions, his organism has been set by the instruction. The instruction in some manner or other determines his subsequent behavior.

The above case is practically identical with that of a child instructed by his mother (*a*) to go to the grocery store

[1] On the possibility of bridging the gap between verbal and non-verbal behavior see a recent study: Watson, J. B., 'The Unverbalized in Human Behavior,' PSYCHOL. REV., 1924, 31, 273–280.

[2] Terman, L. M., 'The Measurement of Intelligence,' 1916, 172–3.

and purchase a loaf of bread, (*b*) then to the butcher shop and ask for the meat, and (*c*) then come home. At the time the child leaves the house his organism has been set by the instruction. A student of behavior would find it very difficult to predict the course of the child's movements if the observer were ignorant of the previous instruction.

(*g*) A further illustration of organic set determined by instruction is found in cases of post-hypnotic suggestion. A suggestion given during hypnosis may express itself later in waking life and the subject may be quite unaware of the existence of the determining set. To account for the phenomenon of post-hypnotic suggestion we must assume the existence of some sort of temporary organic set.[1]

Thus far I have regarded organic set (1) as some sort of peripheral locking of the smooth muscle of *Pecten*, (2) as a posture in the decerebrate cat, (3) as a bodily attitude of the normal animal which orients it with respect to some object, (4) as a neural set which is not immediately manifested by posture, (5) as a neuromuscular or neural set established by verbal instruction or occasioned by non-verbal conditions.

I come now to illustrations of organic set found in the field of experimental psychology.

(*h*) In psychophysical experiments it has been shown repeatedly that the attitude of the subject, as determined by instruction, is an important condition of the subject's report.[2] In order to get unequivocal data the attitudinal set must be controlled. As a matter of fact in nearly every well-ordered psychophysical experiment the subject's set, or *Einstellung*, is controlled.[3]

[1] This point has been made by N. Ach, 'Über die Willenstätigkeit und das Denken,' Göttingen, 1905, 187–191.

[2] The following references will serve as an introduction to the subject in the field of psychophysics: Boring, E. G., 'The Stimulus Error,' *Amer. J. Psychol.*, 1921, **32**, 449–471. 'The Control of Attitude in Psychophysical Experiments,' PSYCHOL. REV., 1920, **27**, 440–452. George, S. S., 'Attitude in Relation to the Psychophysical Judgment,' *Amer. J. Psychol.*, 1917, **28**, 1–37. (See 36–7 for an attempt to define 'attitude.') Fernberger, S. W., 'The Effect of the Attitude of the Subject upon the Measure of Sensitivity,' *Amer. J. Psychol.*, 1914, **25**, 538–543.

[3] Bentley, M., 'The Field of Psychology,' 1924, 384–396. See especially references to Ebbinghaus, Müller and Schumann, Titchener, and von Kries, given in footnotes, p. 388.

The predisposition of the observer may determine the temporal relations of mental processes as seen in Wundt's [1] complication experiment. The temporal order of experience depends upon whether one is set *to see* or *to hear*, and Titchener [2] has put it down as a law of attention that "The stimulus for which we are predisposed requires less time than a like stimulus, for which we are unprepared, to produce its full conscious effect." In experiments upon reaction time [3] the importance of preparatory set is again apparent. Not only the pattern of the reaction consciousness but also the reaction time itself depends upon the *Einstellung* of the subject—whether it be sensory or motor. In the field of perception the illusions of reversible perspective and the various ambiguous figures, such as those of Schumann,[4] illustrate the principle that set is a condition of experience. In the field of thought Ach [5] has noted that there is some determining mechanism capable of selecting out certain associations from various potential associations. Thus, with the instruction *add*, the figures 4 and 2 gave the answer 6; with the instruction *multiply*, the same figures gave the answer 8. In the field of learning it is known that the 'intention' to learn in a particular way is a condition of the learning process. If a subject be asked to associate pairs of nonsense syllables—*a–b; c–d; e–f* . . . ,—he can at a later time reproduce *b* when *a* is given or reproduce *d* when *c* is given. If, however, he be asked to reproduce *c* when *b* is given or to reproduce *e* when *d* is given, the performance is much less complete. The 'set' to learn in a particular way is a condition of performance.[6] It is probably fair to assert that in every field of experimental

[1] Wundt, W., 'Grundzüge der physiol. psychol.' (6th ed.), 1911, 3, 58 *ff.*

[2] Titchener, E. B., 'Lectures on the Elementary Psychology of Feeling and Attention,' 1908, 251. He calls this the 'law of prior entry.'

[3] For a discussion with references to the literature see Titchener, E. B., 'Experimental Psychology,' 1910, Vol. I., Pt. 2, 212–227.

[4] See, for example, Jastrow, J., 'Fact and Fable in Psychology,' 1900, 275–295. I have previously referred to one of Schumann's figures in this connection: Young, P. T., 'The Phenomenological Point of View,' PSYCHOL. REV., 1924, 31, 294–296.

[5] *Op. cit.* See also Titchener, E. B., 'Lectures on the Experimental Psychology of the Thought Processes,' 1909.

[6] Woodworth, R. S., 'A Revision of Imageless Thought,' PSYCHOL. REV., 1915, 22, 18.

psychology evidence can be produced to support the view that some sort of organic set underlies experience and conditions it.

II

In every-day life it is customary to distinguish between physical and mental attitudes, physical attitudes being those which may be described in terms of muscle and nerve, mental attitudes those which may be expressed in words or other symbols. Let us examine the distinction for a moment.

In the first place, it is possible to describe many of the above phenomena in a thoroughly objective way. The bodily posture which an animal assumes may be described in the terms of anatomy and physiology, ultimately perhaps in terms of physics and chemistry. A set may be regarded as a posture of smooth muscle, or as a patterned adjustment of the skeletal muscles depending on nervous integration, or as an integrated neural pattern which persists independently of any peripheral manifestations. Descriptions of this sort are physical and objective.

In the second place, it is possible to state in words or other symbols the meaning of this or that particular instruction. Even if nothing were known about the central physiology of set—and actually very little is known—it would still be possible to approach the problem through a study of the phenomena of instruction. From this point of view we may make such statements as the following: (*a*) organic sets may persist for a brief time or for a relatively long time, (*b*) in many cases an organic set is released when its conscious purpose has been accomplished, (*c*) the maximum complexity of set as tested by instruction varies from individual to individual and in a definite manner with age, (*d*) two or more sets may exist simultaneously and their meanings may be opposed so that conflict results, (*e*) temporary sets may be established through verbal instruction or through non-verbal conditions, (*f*) organic set is a condition of the temporal relations of experience as seen in the complication experiment; it is a condition of reaction time and the pattern of the reaction consciousness; it is a condition of the *Gestalt* of

perceptual experience; it is a condition of learning and of associative recall; it is a condition of behavior.

There are two methods of studying the problem of organic set, but I do not know of any evidence which indicates that there are two kinds of set—mental and physical. Consequently, I prefer to give up the distinction between mental and physical attitudes and to regard the phenomena in question simply as biological. A particular organic set may condition bodily movement, physiologically regarded, and experience, psychologically regarded, at one and the same time. The point is important for students of behavior and for students of experience.

There is a well-known human tendency to substitute teleological interpretation for objective description. The crouching of the tiger expresses the purpose of springing, of capturing and devouring the prey. The phenomenon of tonus in a single muscle, says Sherrington,[1] has no clear purpose when we consider the muscle by itself; but when we consider it as part of a whole it has a purpose—the decerebrate animal stands and the tonus of each individual muscle contributes to this biological purpose.

It seems to me that the phenomena of organic set may be interpreted in terms of mechanism or in terms of purpose or in other ways without changing the phenomena as given. Only further experimental work can throw clear light upon the many problems which the above phenomena suggest.

[1] 'Brain,' *op. cit.*, 193 *f.*

DISCUSSION

THE PROBLEM OF INDIVIDUAL DIFFERENCES IN LEARNING

A recent study by Margaret Kincaid of individual differences in learning throws interesting light upon an old problem.[1] Her study consists in a comparative compilation of a number of other studies, and the chief thing brought out is the unsatisfactory state of affairs with regard to the whole question. Great difficulty was encountered in bringing various results upon the same plane, and it is not clear that this attempt has been altogether successful. However the author is led to conclude that, with an equal increment of practice in a given function, the likelihood of final greater deviation in gross gain is about 50–50; but that decreased deviation in percent of gain is the rule under the same conditions. In practically all cases relative positions were maintained; there were not significant differences in initial and final rank order.

The purpose of the present paper is: first, to point out certain considerations which, when they are not properly taken into account, obscure, and perhaps reverse, the conclusions drawn from studies in individual differences in learning; and second, to suggest that the traditional approaches to problems in this field do not offer much of importance to people interested in learning which involves the higher thought processes.

In this discussion 'initial' will be used for first day and 'final' for last day of the experiment. Strictly speaking, of course, neither is correct. But 'performance' will be used rather than 'capacity,' to designate the measured overt acts. The paragraphs which follow refer directly to Kincaid's study.

1. No mention is made of the mental ages of the subjects. If the subjects in a group are of the same mental age, and if we keep well within physiological limits, the gross gain (in a function positively correlated with mental ability) should be about the same for all; but given different initial performance, the percent of gain will be less for those of high initial performance, and on that basis the deviation will decrease. Initial performance is used as a divisor

[1] Kincaid, M., 'A Study of Individual Differences in Learning,' PSYCHOL. REV., 1925, 32, 34–53.

in both cases; the result among mental equals is that those previously farther along in accomplishment are proportionately penalized. Similarly, equating of any capacity (*e.g.*, speed of movement) correlated with the performance will result in a penalty among capacity equals for those initially furthest advanced.

2. It is possible in such simple material as addition, cross-out tests, etc., that physiological limits are reached, or that the performance at least suffers through distinctly diminishing returns. Suggested possible limits are: visual-motor movements (eye-hand coördination) and capacity for effective attention in monotonous repetition. There may be also lack of motivation for extreme performance. Physical competitions illustrate decreasing returns. For example, the percent of gain represented by a decrease of one second in running 100 yards is assuredly small. Now training a group of unselected men to run 100 yards would probably decrease the deviation in times, measured both from gross gains and percent gains; but the final difference, of say one second, between the best and the worst man would still represent an enormous difference in performance. It would be idle to say that these men have come closer to each other than they were when their respective times were 14 seconds and 12 seconds. The effect of diminishing returns in learning is increased difficulty in adding a unit increment to one's score.

3. Those of lower initial performance may profit by a spurt stage which the higher ones reached before the initial performance. Thorndike calls this the immediate mustering of a group of other skills to the special task in hand, and states that it explains the early rapid rise of the conventional learning curve. If the spurting tendency of the lower group is greater than the tendency of the higher group to excel because of native superiority plus its own (perhaps less) spurting tendency, then the two groups will move closer together, and the final performances will show decreased deviation. That such drawing together will show absolutely about half the time, and relatively every time, is not astonishing. Such differential spurting would be especially likely where marked differences are present in initial performance, and where, at the same time, the upper group is nearing the limit of its capacity.

4. It is not necessarily true that the elements-to-be-learned remain constant throughout the experiment. There is some evidence to the contrary. Thus Ruch states,[1]

[1] Ruch, G. M., 'Correlations of Initial and Final Capacities in Learning,' J. Exper. Psychol., 1923, **6**, 344–356.

The factor of change in mental function under automatization has never been sufficiently recognized in the experimentation on learning. Hollingworth repeated the same list of opposites 175 times with a resulting correlation of —.08 for initial and final abilities. The mental demands of the first and the 175th units must have been very different. Hollingworth recognized this fact himself, it should be noted.

Kincaid's tables show that the raw coefficients of correlation, initial capacity—final capacity, go as low as .15, and that only two studies out of 24 reported a coefficient of .90 or above. Corresponding corrected coefficients go down to .07, and reach .90 or above 9 times out of 24. Hence comparisons made between learning tasks supposedly the same, when there is wide difference in initial performance, and when the initial performances and final performances do not show high positive correlation, are not valid. A unit is not a unit because it represents so-many-a's-crossed-out-per-second any more than 1/5 second is always equivalent to 1/5 second in the 100 yard dash. In some cases the latter difference would be negligible, in others a remarkable performance. Thorndike's 1908 study, in which 33 adults performed multiplication, illustrates the possibility of having two groups fail to compete over any portion of a common learning curve. Kincaid tabulates the following data:

	Gross Gain	% Gain
Av. upper 25%, initial 397, final 703	306	77
Av. lower 25%, initial 158, final 316	158	100

The highest mean point reached by the low group does not reach the initial mean of the high group. That the lower group really made a better showing than the upper group is questionable. But if true, it is relatively unimportant, for as will be pointed out in a subsequent paragraph, that is not the kind of gain we are most concerned with if we hope that the superior group will show greater acceleration than the inferior group.

5. Some of the difficulties encountered by Kincaid are due to an undetermined zero point. If we want to know the relative 'gain' in temperature records, we have to refer to an absolute scale to make it intelligible, even when the units remain constant. 90° C. is not twice as hot as 45° C. It would have been enlightening if some of the investigators had dubbed their 'final' performance 'initial,' and had repeated the same learning procedure from the new starting point. In the higher ranges additional increments of the same kind become increasingly difficult of attainment. A unit of performance which takes this into account is the deviation/

standard-deviation when the S.D. is measured from the mean of a large sample.

6. The *type* of function being studied is an important variable. The work of G. M. Ruch is valuable in this connection.[1] Three types of learning were investigated:

(*a*) Card-sorting, 10 novel designs on 100 cards (50 trials, 10 days);

(*b*) Code-to-English and English-to-code, similar to Year XVI, Test 6, in Stanford-Binet Scale (20 minutes per day, 10 days);

(*c*) Abstract relations, similar to Test 12, Part I, Thorndike Intelligence Examination (15 minutes per day, 10 days).

Correlations of mental age, independent of the factor of chronological age, of the three types of performance were:

$$r\text{M.A.} - (a) = .33, \text{P.E. } .08$$
$$r\text{M.A.} - (b) = .69, \text{P.E. } .04$$
$$r\text{M.A.} - (c) = .73, \text{P.E. } .04$$

Thus it is clear that the behavior of the learning curve described by function (*a*) need not represent that of function (*b*) or (*c*), and that no one of them should be called a *bona fide* example of an over generalized learning curve. As the learning progressed from day to day, a noticeable difference among the above types became increasingly apparent. The correlation between card-sorting and mental age quickly fell to zero; that between code-translation and mental age remained practically constant; that between abstract relations and mental age increased somewhat. Correlations between initial and final abilities were:

(*a*) Card-sorting: .52, P.E. .07
(*b*) Code: Eng.-to-code .78, P.E. .04; code-to-Eng. .61, P.E. .06
(*c*) Abstract relations: rate .78, P.E. .03; accuracy .80, P.E. .03

These values show that considerable change takes place in the function involved. On the other hand certain elements appear to remain constant and, in the case of card-sorting, the constant cannot be mental age. These three types of learning maintained certain differences also when compared on the basis of coefficient of variation, S.D./A.M. For card-sorting there was no change in variability after equal increments of practice; for code-learning the relative variability decreased from day 1 to day 2, and then remained constant; for abstract relations there were ups and downs but no

[1] Ruch, G. M., 'The Influence of the Factor of Intelligence on the Form of the Learning Curve,' to appear in PSYCHOL. MONOG. SERIES.

systematic tendency was observed. As a whole all coefficients remained fairly constant.

Ruch divided his groups on the basis of intelligence into superior, average, and inferior sections. These sections were compared with respect to absolute gains. A noteworthy difference in the type of learning involved was observed. In card-sorting the three sections drew together as learning progressed; in code work the sections made about the same gain; in abstract relations the sections drew farther apart.

It seems clear that no meaningful answer can be given to such blanket questions as: Does added practice reduce original differences? Does the rate of learning depend upon intelligence? Is inheritance or environment the chief factor in learning?

Perhaps two positive suggestions would not be out of place here:

(*A*) Select a group of subjects of the same mental age. Initial performance will be found to vary. Conduct a learning experiment with different types of material. Will the final deviation be greater or less than the initial deviation? In cases where it is less, when percent of gain is used, the differentiating factor cannot be high-group, low-group intelligence conditions (here controlled); in a type of learning which shows a high positive correlation with intelligence, it is more likely a function of differences in initial performance. Some, having previously progressed farther, have provided themselves with larger denominators. One might guess in this instance that the deviation would tend to decrease, on the hypothesis that high initial performance on the same mental age level as low initial performance resulted from fortuitous circumstances.

(*B*) Select a group of subjects of the same initial performance. Mental age will be found to vary. Conduct a learning experiment with different types of material. Will the final deviation in each case be greater or less than the initial deviation? Note that the denominator in figuring the percent of gain is now the same for all. The deviation, being zero, must either remain so, or increase. For types of learning showing high positive correlation with intelligence one might guess that the deviation would increase markedly; that subjects of higher mental age (or perhaps of higher I.Q. among children) would increasingly draw away from the others. Similarly a fruitful study might be made holding 'final' performance constant; *i.e.*, allowing the number and the length of the practice periods to vary. The present writer feels sure that it would not require

great ingenuity to set up learning experiment conditions such that the experimenter may honestly report less deviation, equal deviation, or greater deviation, at will, as a result of his investigations.

The second purpose of this paper is to point out one conclusion, or rather assumption, that surely does not follow from the type of experiments considered by Kincaid, but that offers a tempting generalization for those interested in learning which involves the higher thought processes. Should college students maintain different rates of performance and improvement in their work in accordance with their mental abilities? This question must be answered by working in the field in which these processes function, not by extension of findings in conjunction with simpler mental processes. Probably the effect of a system commonly found—one class, one assignment, one grading system, one type of motivation, one performance-standard—tends to lessen the original dispersion; a number of the dull students work hard to 'get by,' and a number of the bright ones work a little and also 'get by.' An investigation at the University of Iowa in 1923, based on 214 students, showed that there was no correlation between the time devoted to studying and the number of grade-points earned. This could not be true if the best students also worked the most. Improvement of the kind described in the studies reported by Kincaid does not apply to university learning problems. The problem is not one, ordinarily, of doubling the present speed of addition, or of crossing out letters, or of discovering what type of motivation is needed to produce such improvement. The superior performance of superior students will be measured in part in ability to grasp more content, but it will be related content, different content, rather than just more of the same thing. He need not know the simpler things any better than the average student does, but he will know things in a coördinated way; he will have woven his knowledge into an integrated fabric of great potentiality. It will not make much difference how excellent his memory is, or how perfect in detail his coördinated material, so long as he develops and maintains an appetitive-digestive-productive mental system devoted to the ends set up as the goal of university training. It is important to know the intricacies of his mental-educational relationships, what area he can cover; it is even more important to know the depth of these relationships, how high he can go. It is this hierarchical feature of learning that learning experiments carried out in linear fashion with simple material completely miss. It is the difference

between measuring the learning of an expert typist and the learning of an expert secretary. It is to be hoped that superior students will maintain their lead, or increase it, as learning advances; but maintenance of this lead will depend upon more complete mastery of things more abstract, rather than upon more complete mastery of things within the range of all.

Miss Kincaid's excellent study has brought out a rather unexpected heterogeneity of conclusions to be drawn from many of the best known learning experiments. It has been shown that probably none of them can be accepted at face value; and that, moreover, what agreements they have come to, do not apply in the higher reaches of learning.

While this paper has suggested several specific criticisms of procedure, the fundamental issue is that, no matter how exact the procedure may be, the results of the experiment should not be made the basis of conclusions about learning of a higher type, or about learning in general. To build up our knowledge of complex integrated learning acts, we must study these acts directly.

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